China and the World Bank – How Contrasting Development Approaches Affect the Stability of African States

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Appendix

Table of Contents

A Data Appendix

A.1 Outcome Variables

SCAD's level of detail on event types makes it a valuable dataset for analyzing government repression. First, "Pro-Government Violence (Repression)" is defined as violent events proactively *initiated* by the government itself or groups in explicit support of the government directed at individuals or groups. Second, the dataset categorizes each event by *government response*. That is, peaceful or violent events triggered no repressive, lethal repressive, or non-lethal repressive government response. SCAD considers a government response as repressive if tear gas has been used, people have been arrested, or employed similar tactics. Similarly, lethal government responses follow the same definition but subsets events with reported deaths of the event. The analysis includes both types of repression (initiated and response) for all SCAD events except for intra-government violence since this type captures coups and clashes among government entities.

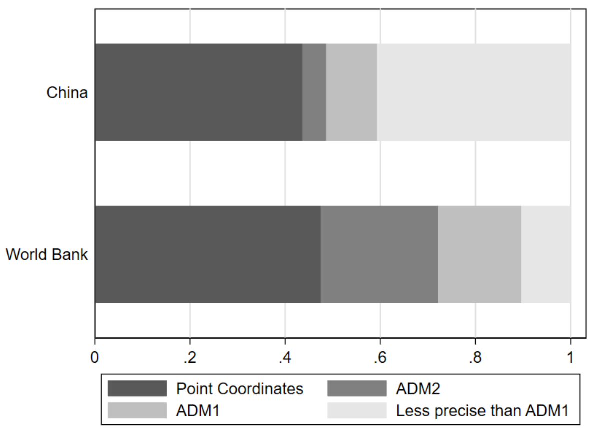
A.2 Independent Variables (Development Aid)

WB's IDA & IBRD Disbursements

For our analysis, we draw on the "WB IBRD-IDA, Level 1, Version 1.4.1" provided by the AidData consortium, which covers approved loans under the IBRD-IDA lending line between 1995 and 2014.[12] These data correspond to project aid disbursed from 5,684 projects in 61,243 locations. The data builds on the information provided by the WB, including the disbursement dates, project sectors, and disbursed values. These values are deflated to 2011 values. In an effort to allow for more fine-grained analysis of aid projects, AidData's coders filtered the location names from aid project documentation and assigned these to specific locations. Some projects include exact locations on latitude and longitude. Other projects, which had a more policy or regulation-oriented purpose, could only be assigned to an administrative level (e.g., the first-level administrative division (provinces) or the second level (districts). To include as many disbursements as possible, but to also grasp the advantages of georeferenced data, we focus our analysis on these administrative levels. For our administrative boundaries, we build on the GADM dataset constructed by Hijmans et al. (2010). One difficulty with this data is that more fine-grained administrative distinctions are missing for some countries. As the size of administrative regions is not fixed by size across countries, we assume in these cases that our ADM1 regions would be ADM2 regions.

Figure 7 displays the development finance locations coded by donor, distinguishing all projects (precision 1-8), projects coded at least at the first administrative level (precision 1-4), projects coded at least at the second administrative level (precision 1-3), and projects coded more precisely with exact point coordinates (precision 1-2).

Figure 7: No. of Project Locations by Precision Codes



Notes: Based on WB and Chinese project aid data from Strange et al. (2017) and Dreher et al. (2019).

One challenge arises in projects with many locations, where we do not know the distribution of the overall value of disbursements across locations. In this regard, we suggest two solutions.

[12]Since the number of documented projects declines steeply after 2012, we focus on the 1995-2012 period.

First, we allocate disbursements by the number of locations. In line with previous research by Dreher and Lohmann (2015), we assume that aid is distributed equally across locations and allocate aid proportionally to the locations per region. For instance, for a project with 10 locations, where 4 locations are in region A and 6 locations are in region B, 40% of project disbursements would be accounted in region A and 60% in region B.

Second, we calculate population-weighted disbursements. Here, we assume that aid is allocated based on the regional population shares. For instance, if a project would have project locations in two regions of a country, where two million inhabitants would reside in region A and three million would reside in region B, 40% of project disbursements would be accounted in region A and 60% in region B. Here, the aid attribution formula would write as follows: $Aid_{pijt} = \frac{Aid_{pit}}{\int Population_{pi}} * Population_{pj}$, where p is the project, i is the country, j is the region, and t is the period for which we estimate the allocation shares.

Finally, our dataset comprises development finance from IBRD and IDA. However, only IDA disbursements classify as Official Development Assistance. For this purpose, disbursements are disentangled into IDA (development aid) and IBRD (development finance) disbursements.

Allocation Scheme (more detailed)

Location Weighting

The WB geocoded data release comes in the format of projects and several corresponding locations. For instance, a typical project report would mention the transaction amounts, the project purpose, and different project locations. The latter can be classified in different degrees of precision (e.g., precision codes smaller than 4 correspond to locations that refer to an ADM2 region or even more precise, while precision code 4 corresponds to locations at the ADM1 level). When allocating the development aid across locations on the ADM1 and ADM2 levels, we make the following assumptions based on a three-step procedure.[13] First, we subtract the share of development aid, which corresponds to locations, which are coded less precisely than ADM1 (e.g., large geographic regions or aid at the country level). For example, if three out of 10 locations in a project are coded less precisely than ADM1, the further analysis focuses on the remaining 70% of development aid. Second, we then allocate all aid with precision codes 1-3 to the corresponding ADM2 regions. This is done by taking the location share (either by equal or population weights) of the transaction amount per location. A certain ADM2 region may have several locations per project or even several projects; we collapse our data by ADM2 region. Third, we then allocate all aid with precision code 4 to the corresponding ADM1 regions. This is done by taking the location share (either by equal or population weights) of the transaction amount per location. Because certain ADM1 region may have several locations per project or even several projects, we collapse

[13]Throughout the paper, we allocate the aid either assuming equal weights per location or weighting each location by population.

our data by ADM1 region. To allow for inference on the ADM2 level, we assume that transactions coded with precision 4 are attributable equally to all corresponding ADM2 regions. In practice, this is done by merging the ADM1 regions with all corresponding ADM2 regions and then splitting the aid with location or population weights. Finally, data with precision codes 1-3 and precision code 4 can be simply added upon the ADM2 level yielding our treatment variable of interest. For inference on the ADM1 level, totals of ADM2 level development assistance are created on the geo-unit-year level.

Table 4: Weighted Aid Allocation Formula Example

ID	Year	Aid Value	Loc. ID	ADM1 ID	ADM2 ID	Prec. Code	ADM1 Weight	Prec.4 Aid to ADM2	Prec. 1–3	Total Aid
	1995	100	2	1	1	1	1/7		14.29	14.29
1	1995	100	3	1	2	2	1/7		14.29	14.29
1	1995	100	4	2	1	4	1/7	14.29		14.29
1	1995	100	5	3	1	3	1/7		14.29	14.29
1	1995	100	6	3	2	1	1/7		14.29	14.29
1	1995	100	6	3	3	4	(1/7)*(1/3)	4.76		4.76
1	1995	100	6	3	1	4	(1/7)*(1/3)	4.76		4.76
1	1995	100	7	3	2	4	(1/7)*(1/3)	4.76		4.76
1	1995	100	8	4	1	4	1/7	14.29		14.29
Totals:								42.86	57.14	100.00

Population Weighting

Analogous to the location weighted aid, we also distribute aid with population weights. Our population data are from the CIESIN (2016). However, some projects only consist of locations without population estimates (e.g., deserts). In this case, we assume a population of one citizen per location to be able to distribute those aid disbursements. We then consequently attribute the population of ADM1 regions to project locations, which are coded at the ADM1 level (precision 4), and ADM2 populations to project locations, which are coded at least as precise as the ADM2 level (precision 1-3).

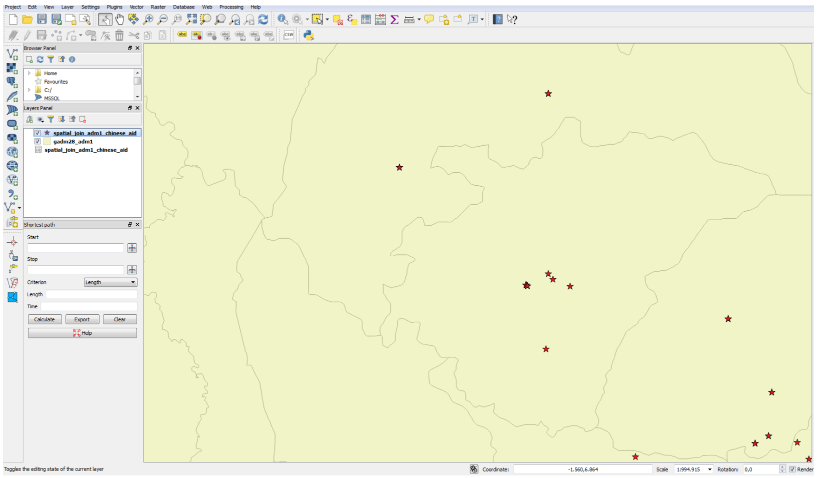
Similar to the location-weighing, we construct the total population of each project-year $pop_{project}$. For the projects coded with precision 4, we then attribute disbursements via the regional share in population pop_{ADM1}. This is then divided by $pop_{project}$ and multiplied with the project disbursements $TransactionValue_{proj}$ in each year: $ADM1Precision_4 = \frac{pop_{ADM1}}{pop_{proj}} * TransactionValue_{proj}$. Since there may be several active projects per ADM1 region, we aggregate the disbursements on the ADM1 level. In order to break those numbers down to the ADM2 level, we merge all corresponding ADM2 regions to the ADM1 regions. We then divide the population in each ADM2 region by the population in each ADM1 region and multiply this share with the yearly disbursements per region, $ADM2Precision_4 = \frac{pop_{ADM2}}{pop_{ADM1}} * ADM1Precision_4$. For the precision codes 1-3 (at least coded as precise as the ADM2 level), we then attribute disbursements via the regional share in population divided by $pop_{project}$.

This is then multiplied with the project disbursements in each year: $ADM2Precision_{123} = \frac{pop_{ADM2}}{pop_{proj}} * TransactionValue_{proj}$. Because there may be several active projects per ADM2 region, we aggregate the disbursements on the ADM2 level. Finally, we merge the precision code 1-3 and 4 data on the ADM2 level to obtain our variables of interest. Those can then be aggregated on the ADM1 level.

Chinese Aid (ODA-like and OOF flows)

To create our data on the ADM2 and ADM1 levels, we use the feature that aid can be defined on the ADM2 level and then aggregated to the ADM1 level. However, one challenge with the data is that we lack information on the ADM2 regions for some countries (as there are no ADM2 regions in small countries). Therefore, we create two spatial joins of ADM1 and ADM2 regions from the GADM dataset with Chinese aid point features. This yields matches of the specific project locations with the administrative regions, as depicted in Figure 8.

Figure 8: Chinese Aid Spatial Join at ADM1-level



Notes: The figure depicts the information on locations of Chinese aid projects (stars) within administrative boundaries, which we use for spatial matching. Graphical depiction based on Quantum GIS.

To create our data, we first load our ADM2 data into Stata and drop the ADM0 and ADM1 identifiers to be later able to rely on the identifiers from the ADM1-Aid spatial join. The next step involves merging the ADM2-Aid spatial join with the ADM1-Aid spatial join by the target-fid, which uniquely identifies the points from the Dataset "aiddata_china_1_1_1.xlsx" by Dreher et al. (2019) and Strange et al. (2017). Based on this data, we create unique identifiers for all ADM1 and ADM2 regions, whereby we treat ADM1 regions as ADM2 regions in cases that ADM2 regions are missing (e.g., in Cape Verde). This assumption can be made as sizes of administrative regions are somewhat arbitrary, and several ADM2 regions are larger than other countries' ADM1 regions. After getting the regional identifiers right, we can merge (a) the spatial joins of ADM regions and Chinese aid locations with (b) data on flows of Chinese aid. In the first step, we clean these data from entries that only relate to pledges of Chinese aid (information is from the variable status254). Although the data on Chinese finance to Africa also contain information on official investment, the focus of this paper is on development aid. Thus, we focus on flows, which correspond to "ODA-like" funds as those would compare closest to development aid (following individual correspondence with the authors of Strange et al. (2017)). The data are then merged with population data from the gridded population of the world data to allocate financial flows with population weights in case one project had commitment locations in different administrative regions. Yet, one further challenge has to be resolved before allocating the commitments to regions. The Chinese aid commitments are coded like WB disbursements with different precision (e.g., some are coded only for geographic features. Such aid involves several administrative regions or funds that go to central ministries or the government). For our commitment allocation, we only consider those projects, which are at least coded at the ADM1 level. This means that we proportionally exclude commitments, which provide information on the central level and sub-regional levels as indicated before. Furthermore, we distinguish between projects, which are coded only at the ADM1 level, and those that provide information on the ADM2 level (or more precisely). The former are proportionally split over the underlying ADM2 regions. Although the latter can be precisely traced back to the ADM2 region, projects may have commitments in several ADM2 regions. In this case, we also split the commitments proportionally by locations or population, as indicated earlier.

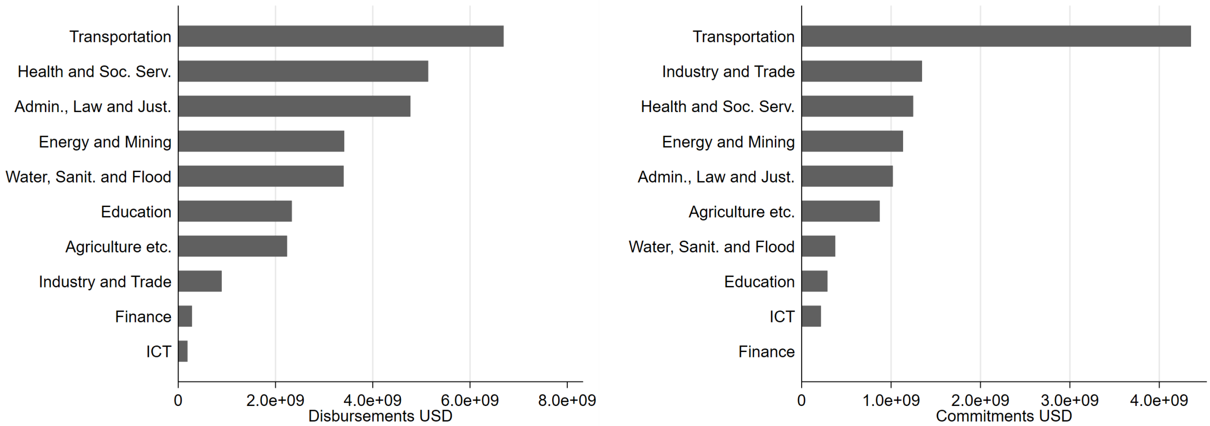
To exploit sectoral variation in development finance both for the WB and China, we make use of the information provided by Strange et al. (2017) on Chinese aid's sectoral allocation using the OECD's Creditor Reporting System (CRS) codes. To achieve comparability with the broad sectors indicated for the WB, we assign sectors as follows: "Agriculture, Fishing and Forestry" (CRS-310:"Agriculture, Forestry and Fishing "), "Public Administration, Law, and Justice" (CRS-150), "Information and communication" (CRS-220: "Communications"), "Education" (CRS-110: "Education"), "Finance" (CRS-240: "Banking and Financial Services"), "Health and other social services" (CRS-120: "Health," CRS-160: "Other Social infrastructure and services"), "Energy and mining" (CRS-230: "Energy Generation and Supply"), "Transportation" (CRS-210: "Transport and Storage"), "Water, sanitation and flood protection" (CRS-140: "Water Supply and Sanitation"), and "Industry and Trade" (CRS-330: "Trade and Tourism," CRS-320: "Industry, Mining, Construction").

Sectoral Distribution of Aid Disbursements

We use additional information on the financier for each disbursement for each project. Based on this information, we can construct sectoral distributions of aid flows. While both donors are investing heavily in transportation across Africa, further priorities differ. The WB strongly supports Health and Social Services, whereas China commits a large share of its funds to Industry and Trade. We account for the heterogeneity in the aid portfolio by running regressions for sectoral aid in the analytical appendix Table A29.

Figure 9: Sectoral Distribution of Aid

(a) WB's IDA   (b) China



Chinese Aid (Compilation and Potential Reporting Bias)

Strange et al. (2017) compiled information on Chinese aid data from media reports, among other sources. The authors build on the TUFF methodology, which covers a broad set of quality and triangulation steps. This includes further comparisons with reports by NGOs and academics as well as information provided by Chinese government websites and recipient governments, when available. However, due to the partial reliance on media, one may be afraid that politically controversial and conflict-prone projects would be under-reported in regimes with low press freedom (Kilby, 2017). To consider this issue, we provide descriptive statistics which distinguish between countries with low/high press freedom. Table A5, therefore, considers whether administrative regions with a history of conflict are characterized by a lower reported amount of development aid and whether there is a systematic difference across countries with a low/high press freedom according to Freedom House (2021). Descriptive statistics indicate that regions with a history of conflict receive less aid. However, the ratios of the amount of aid committed to peaceful and conflict-affected regions do not differ substantially when comparing countries with low press freedom (0.833) to high press freedom (0.815). However, comparable ratios between aid to conflict and non-conflict regions, irrespective of low/high press freedom, suggest no systematic media bias.

Table A23 considers this notion more rigorously for the main outcome, where we add the interaction of press freedom and region-level fixed effects as a further endogenous control. Table A30 repeats this exercise for the different conflict outcomes. Coefficients remain insignificant and are comparable to the main results in Table 3 suggesting that press freedom would not systematically bias aid reporting and the relationship between aid and conflict.

Table 5: Conflict History and Reporting of Chinese Aid

		Conflict History		
		No	**Yes**	**Ratio**
Press Freedom	**Low**	2,112,100 USD	1,760,282 USD	*0.833*
	High	785,501 USD	640,210 USD	*0.815*

Notes: Conflict history is a dummy variable which equals one if at least one previous year had a conflict incidence of ≥ 5 Battle-related Deaths. The ratio is obtained by dividing the average aid amount committed (in USD) to regions with no prior conflict by the value for regions with a history of conflict.
Source: Author's calculation based on Strange et al. (2017), Dreher et al. (2019), Croicu and Sundberg (2015) and Freedom House (2021).

A.3 Dependent Variables (Conflict Data)

Table A.3 provides an overview of the different conflict outcomes considered in this paper. The construction of the data and sources are described in more detail in the subsequent paragraphs.

Table 6: Descriptive Statistics - ADM1 Region

	Mean	SD	Min	Max
Conflict Incidence	11.63	32.06	0	100
State Based Conflict	7.07	25.63	0	100
Non-State Based Conflict	3.75	18.99	0	100
State Violence vs Civilians	1.86	13.52	0	100
Non-State Violence vs Civilians	3.34	17.97	0	100
Riots, Strikes & Demonstrations	13.83	34.52	0	100
Riots	8.27	27.54	0	100
Strikes	7.61	26.51	0	100
Demonstrations	2.97	16.98	0	100
Non-lethal Government Repression	7.37	26.14	0	100

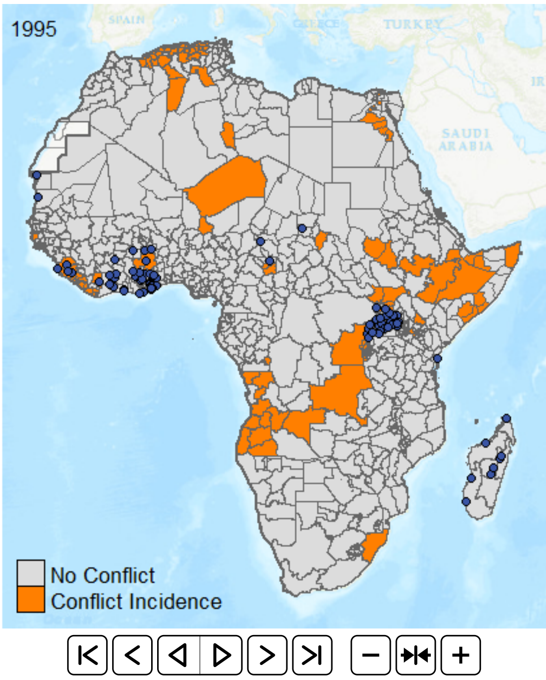
Notes: Descriptive statistics for our main outcome variables. The sample period is 1995-2014 in order to account for the different lag structures. Click here to go back to section 3.2.

UCDP Data

AidData and UCDP use the same coding framework, so we can use similar coding rules and restrict us to events coded at least at the ADM1 level (precision codes 1-4). For the more precise data (precision codes 1 and 2), we again use a point to polygon analysis on the ADM level. Since one conflict event is always coded in one discernible location (Croicu and Sundberg, 2015), we do not need to make additional distributional assumptions by location number or population size for conflict data because we do not face issues of multiple project locations, which we had in the aid data. Yet, for conflict observations on the ADM1 level (precision code 4), we do not distribute Battle-related Deaths by population weights across ADM2 regions.

A useful feature of the UCDP data is the possibility to discern three different types of violence. Those are the government against organized groups (type 1), organized non-governmental groups versus the government (or against another non-governmental group) (type 2), and one-sided violence by the government against civilians (type 3 governmental) and by non-governmental groups against civilians (type 3 non-governmental).[14]

Figure 10: WB Aid and Conflict - By Year



Notes: The figure depicts the locations of World Bank aid projects (blue dots) within administrative boundaries. Sub-national administrative regions, which experience outright conflict with more than or equal to five Battle-related Deaths in a given year, are shaded in orange.

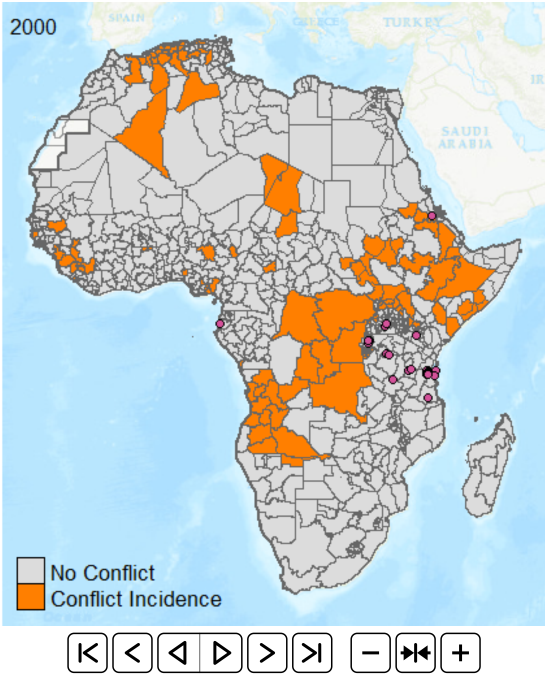
UCDP data can be considered as comprehensive for our 1995 to 2012 sample. Hence, all missing values are treated as zeros. For Syria, information on Battle-related Deaths is not reported, and the country is, thus, not part of our analysis.

SCAD Data

UCDP data focus on organized violence with lethal outcomes. However, along with the different theories, it could be hypothesized that discontent and aid appropriation do not necessarily need to be linked to full-fledged conflict. What is more, recent empirical work by Bluhm et al. (2021) underscores the role of aid in conflict dynamics. Thus, we also consider

[14]Please consult Croicu and Sundberg (2015) for a detailed description of the different violence types.

Figure 11: Chinese Aid and Conflict - By Year



Notes: The figure depicts the locations of Chinese aid projects (pink dots) within administrative boundaries. Sub-national administrative regions, which experience outright conflict with more than or equal to five Battle-related Deaths in a given year, are shaded in orange.

social conflict as a further outcome, in terms of demonstrations and repressions, based on the Social Conflict Analysis Database (Salehyan et al., 2012).[15]

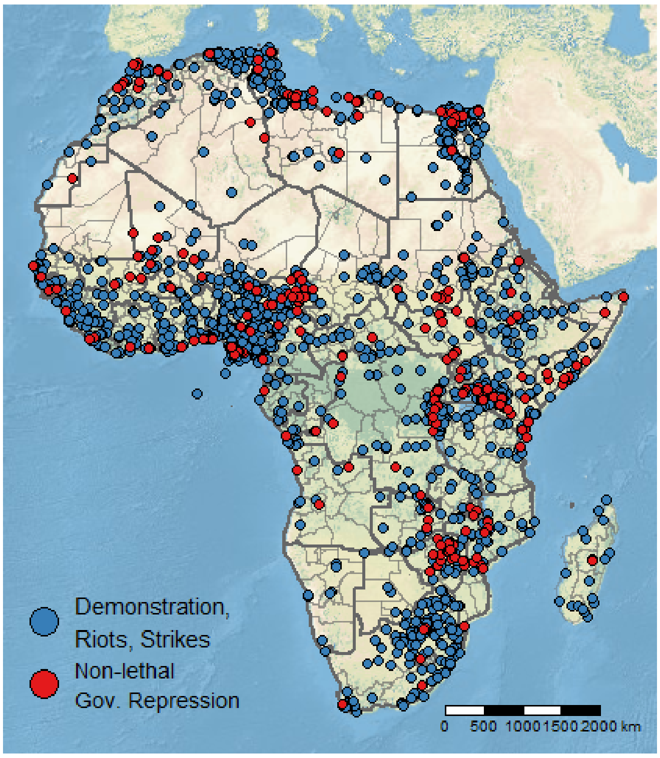
SCAD involves demonstrations, riots, strikes, coups, pro-, anti-, and extra-government violence, which can but do not necessarily have to involve casualties. This way, SCAD complements the UCDP data.[16] SCAD mainly builds on data compiled by the Lexis-Nexis services from searches of Agence France Presse and Associated Press. Based on the available information, data are georeferenced by web searches of the locations mentioned in the event reports. Analogous to UCDP data, precision codes are provided, which are used to allocate events similarly.

SCAD's level of detail on event types makes it a valuable dataset for analyzing government repression, which we consider a further potential outcome of aid allocation. First, "Pro-Government Violence (Repression)" is defined as violent events pro-actively *initiated* by the government itself or groups in explicit support of the government directed at individuals or groups. Second, the dataset categorizes each event by *government response*. That is, peaceful or violent events triggered no government response or repressive government response. In combination with the number of fatalities, we are able to distinguish a repressive government

[15]Non-lethal repression events in the SCAD database range from the repression of opposition lawyers to constraining anti-government artists in Egypt and media restrictions in Malawi, but also use of tear-gas against demonstrators. (Salehyan et al., 2012).

[16]Prior to 2014, armed conflict was not included in SCAD and is now distinguished from "social disturbances."

Figure 12: SCAD Data for Precision Codes 1-4



response to social unrest events into lethal repression and non-lethal government repression. SCAD considers a government response as repressive if tear gas has been used, people have been arrested, or employed similar tactics. The analysis includes both types of repression (initiated and response) for all SCAD events except for intra-government violence since this type captures coups and clashes among government entities.

A.4 Dependent Variables (Afrobarometer)

Measures on people's norms about democracy are taken from Afrobarometer Data (2018). The geocoded individual responses are matched with the administrative region and the response values to the respective questions are averaged on the first administrative level to allow matching with regional aid flows.

Table 7: Afrobarometer - Labels, Questions and Sources

Variable Name	Variable Description	Availability	Code
Panel A: Security			
Security facilities: Police station present within walking distance?	Are the following facilities present in the primary sampling unit/enumeration area or within easy walking distance: Police station?	2008-2009, 2011-2014	ea-fac-c
Security forces: Any policemen or police vehicles?	Are the following facilities present in the primary sampling unit/enumeration area or within easy walking distance: Police station?	2008-2009, 2011-2014	ea-sec-a
Security forces: Any soldiers or army vehicles?	In the PSU/EA, did you (or any of your colleagues) see: Any soldiers or army vehicles?	2008-2009, 2011-2014	ea-sec-b
Frequency of things stolen in the past year?	During the past year, have you or anyone in your family: Had something stolen from your house?	2002-2006, 2008-2009, 2011-2014	q11a-x
Frequency of physical attacks in the past year?	During the past year, have you or anyone in your family: Been physically attacked?	2002-2006, 2008-2009, 2011-2014	q11b-x
Panel B: Democratic norms and attitudes			
Democracy: How democratic is your country today?	In your opinion, how much of a democracy is your country today?	1999-2006, 2008-2009, 2011-2014	q40
Democracy: Did you perceive last elections as free and fair?	On the whole, how would you rate the freeness and fairness of the last national election held in your country?	1999-2001, 2005-2006, 2008-2009, 2011-2014	q22-x
Governance: Reject one-party rule	There are many ways to govern a country. Would you disapprove or approve of the following alternatives: Only one political party is allowed to stand for election and hold office?	1999-2006, 2008-2009, 2011-2014	q28a
Governance: Reject military rule	There are many ways to govern a country. Would you disapprove or approve of the following alternatives: The army comes in to govern the country?	1999-2006, 2008-2009, 2011-2014	q28b
Governance: Reject one-man rule	There are many ways to govern a country. Would you disapprove or approve of the following alternatives: Elections and Parliament are abolished so that the president can decide everything?	1999-2006, 2008-2009, 2011-2014	q28c
Reject government banning organizations that go against its policies	Which of the following statements is closest to your view? Choose Statement 1 or Statement 2. Statement 1: Government should be able to ban any organization that goes against its policies. Statement 2: We should be able to join any organization, whether or not the government approves of it.	2005-2006, 2008-2009, 2011-2014	q16-x
Panel C: Government responsiveness and repression			
Frequency of contact to government official to express your view	During the past year, how often have you contacted any of the following persons about some important problem or to give them your views: An official of a government agency?	1999-2006, 2008-2009, 2011-2014	q24c-x
Fear of political intimidation or violence during campaigns	During election campaigns in this country, how much do you personally fear becoming a victim of political intimidation or violence?	2008-2009, 2011-2014	q49-x
How often do people have to be careful about what they say in politics?	In your opinion, how often, in this country: do people have to be careful of what they say about politics?	2002-2006, 2008-2009, 2011-2014	q51a-x
Rule of Law: People must obey the law	For each of the following statements, please tell me whether you disagree or agree: The police always have the right to make people obey the law.	2002-2006, 2008-2009, 2011-2014	q42b
Frequency of joining others to request government action	Here is a list of actions that people sometimes take as citizens when they are dissatisfied with government performance. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Joined others in your community to request action from government.	2014	q27a

Table 8: Afrobarometer - Questionnaire Rounds and Countries

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Algeria	–	–	–	–	2013	2015
Benin	–	–	2005	2008	2011	2014
Botswana	1999	2003	2005	2008	2012	2014
Burkina Faso	–	–	–	2008	2012	2015
Burundi	–	–	–	–	2012	2014
Cameroon	–	–	–	–	2013	2015
Cape Verde	–	2002	2005	2008	2011	2014
Cote d'Ivoire	–	–	–	–	2013	2014
Egypt	–	–	–	–	2013	2015
Ethiopia	–	–	–	–	2013	–
Gabon	–	–	–	–	–	2015
Ghana	1999	2002	2005	2008	2012	2014
Guinea	–	–	–	–	2013	2015
Kenya	–	2003	2005	2008	2011	2014
Lesotho	2000	2003	2005	2008	2012	2014
Liberia	–	–	–	2008	2012	2015
Madagascar	–	–	2005	2008	2013	2015
Malawi	1999	2003	2005	2008	2012	2014
Mali	2001	2002	2005	2008	2013	2014
Mauritius	–	–	–	–	2012	2014
Morocco	–	–	–	–	2013	2015
Mozambique	–	2002	2005	2008	2012	2015
Namibia	1999	2003	2006	2008	2012	2014
Niger	–	–	–	–	2013	2015
Nigeria	2000	2003	2005	2008	2013	2015
Sao Tome/ Principe	–	–	–	–	–	2015
Senegal	–	2002	2005	2008	2013	2014
Sierra Leone	–	–	–	–	2012	2015
South Africa	2000	2002	2006	2008	2011	2015
Sudan	–	–	–	–	2013	2015
Swaziland	–	–	–	–	2013	2015
Tanzania	2001	2003	2005	2008	2012	2014
Togo	–	–	–	–	2012	2014
Tunisia	–	–	–	–	2013	2015
Uganda	2000	2002	2005	2008	2012	2015
Zambia	1999	2003	2005	2009	2013	2014
Zimbabwe	1999	2004	2005	2009	2012	2014

Source: Afrobarometer Data (2018)

A.5 Identifying Ethnic Power Relations

Matching EPR to GREG

To measure ethnic homelands, we use the GREG dataset (Weidmann et al., 2010). It is a georeferenced version of the initial locations of ethnic homelands based on the Soviet Atlas Narodov Mira. The information about the power status comes from the time-variant Ethnic Power Relations (EPR) dataset (Vogt et al., 2015). Wherever possible, we match the group power status from EPR in a particular year to one of the time-invariant GREG group homelands. The original dataset assigns eight different power statuses to groups. The differences are sometimes marginal and hard to interpret. To minimize measurement error, we only use the more precise information on whether a group was part of the governing coalition or not. We then intersect the ethnic group polygons with the administrative regions to classify regions as one of the three categories. For our application in Figure 6, we employ a measure which equals one if all groups were part of the power sharing agreement at the central level and is equal to zero if at least one group was excluded.

A.6 Sources

Table 9: Data Sources

Variable Name	*Variable Description*	Time Period	*Variable Source*
WB Aid	log of WB Aid disbursements per region-year	1995-2012	Strandow et al. (2011)
Chinese Aid	log of Chinese Aid commitments per region-year	2000-2012	Dreher et al. (2021b)
Strikes, Riots, Demonstrations	Binary indicator (100;0) if any violent event of this type in a given region-year took place	1995-2012	Salehyan et al. (2012)
Intensity 1/2 conflict	Binary indicator (100;0) 1 if >=5/>=25 persons were killed in a given region-year	1995-2014	Croicu and Sundberg (2015)
Population	Continuous indicator of regional population	1995-2014	(CIESIN 2016)
Drought (end of rainy season)	SPI value of drought severity of the region's rainy season	1995-2014	Tollefsen et al. (2012); Guttman (1999)
Drought (start of rainy season)	SPI value of drought severity during the first month of the region's rainy season	1995-2014	Tollefsen et al. (2012); Guttman (1999)
Temperature	Mean temperature (in degrees Celsius) per region-year	1995-2014	Tollefsen et al. (2012); Fan and Van den Dool (2008)
Precipitation	Total amount of precipitation (in millimeter) per region-year	1995-2014	Tollefsen et al. (2012); Schneider et al. (2015)
Elevation	Standard deviation of regional elevation as an indicator of ruggedness of terrain	Constant	USGS Global 30 Arc-Second Elevation (GTOPO30)
Ocean, Rivers, Lakes	Binary indicator of region's presence of rivers, lakes or ocean	Constant	Natural Earth, from Natural Earth.com
Land area	Area of a given region	Constant	Hijmans et al. (2010)
Travel Time (Mean)	Gives the mean regional estimate of the travel time to the nearest major city	Constant	Tollefsen et al. (2012); Uchida and Nelson (2009)
Borders	Binary indicator if a region borders another country	Constant	Own estimations based on Hijmans et al. (2010)
Chinese Commodity	Chinese commodity production (factor, standardized)	1999-2013	Bluhm et al. (2020a); Dreher et al. (2021b)
IDA Funding Position	WB's net investment portfolio & demand obligations divided by undisbursed commitments.	1995-2012	Dreher et al. (2021b)

B Instrumental Variables

B.1 Instrumental Variable Approach

Beyond conflict, there is emerging and growing literature on the effect of Chinese aid on various outcomes. Some of those use an event-study approach like Dreher et al. (2019) or a spatial Difference-in-Differences (DiD) estimation (Isaksson and Kotsadam, 2018a,b). Many prominent studies also employ an instrumental variable approach, which considers the heterogeneous impact of a plausibly exogenous time series affecting the amount of aid allocated, depending on a pre-determined cross-sectional difference in the probability of receiving aid. This strategy is based on Nunn and Qian (2014) and Dreher et al. (2021b), and has been applied to Chinese and other aid by Bluhm et al. (2020a); Dreher et al. (2021a). While our approach for the main paper emphasizes a FE approach for reasons of transparency and clarity, we want to show how our main results would look like when such an IV approach is used.[17]

The first stage of the IV approach builds on the following structure

$$Aid_{i,t} = \alpha_1 p_{i,t-1} + \alpha_2 TimeSeries_t + \alpha_3 p_{i,t-1} TimeSeries_t + \epsilon_{i,t-1}, \tag{4}$$

where the probability $p_{i,t-1}$ is computed by dividing the number of years a region i has received aid by the number of years passed until year $t-2$.[18,19] As in any DiD setup, both regression stages control for the main constituting terms forming the interaction; only the interaction term is used as the conditionally exogenous instrument in the first stage. The identifying assumption is that there would be common trends in aid allocation in low and high aid probability recipient regions in the absence of a change in the time series. The IV for WB aid and Chinese aid rely on the same idea but differ in the donor-specific probability and the factor $TimeSeries_{t-1}$ that introduces variation over time.

There are some similarities of such an approach to the popular Bartik-style and shift-share instruments, which are common in the trade or migration literature (e.g Kovak, 2013; Jaeger et al., 2018). In both cases, a cross-sectional term that could be endogenous or pre-determined is interacted with a plausible exogenous time series. If location and time fixed effects capture the first two main terms, our instrumental variable part for the aid first stage will read:

[17] See also the working paper version of this paper, (Gehring et al., 2019).

[18] If beginning in 1995, and a region received aid in three out of five years, the aid probability in 1999 would be 0.6. If aid receipts stop in 1999, the probability declines to 0.5 in 2000 as the region received aid in three out of six years. Nizalova and Murtazashvili (2016) show that if the heterogeneity of interest (here the probability of receiving aid) is independent of the treatment (here the donor's global aid budget), the interaction of exogenous and endogenous variables can be interpreted as exogenous when controlling for the endogenous factor (in this case the probability to receive aid). Using initial or pre-determined values allows us to relax these assumptions, compared to using a constant probability as in Nunn and Qian (2014) or (Bluhm et al., 2020a).

[19] Nunn and Qian exploit temporal variation in US wheat production, interacted with a constant probability to receive US food aid.

$$Aid_{i,c,t} = p_{i,t-1} TimeSeries_t \tag{5}$$

Using a similar notation, the equation for a Bartik-style IV usually resembles

$$x_{i,t} = \sum_{\kappa=1}^{K} p_{i,k,0} \times TimeSeries_{k,t}(6)$$

where the $p_{i,k,0}$ stands for either an initial or lagged industry share (trade literature) or country of origin (migration literature), and the $TimeSeries$ could be industry- or country-specific exogenous changes.

While for Bartik and shift-share instrument, there are several industries or countries of origin, the application to development aid considers only one cross-sectional dimension (e.g., country-regions) and a single time series variable. Thus, one cannot compute the weights and influence of different individual dimensions (industries, countries of origin). Nonetheless, some assumptions are similar to the multi-dimensional shift-share approach.

Hence, some of the critiques and potential robustness tests also apply to our setting.

We begin by describing how we adapt the approach for the WB and China in the following section.

B.2 Application to WB and China

B.2.1 Application to WB Aid

Based on discussions with WB staff and recipient country personnel, the mechanism we exploit and document for identification is the following. The availability of additional "free" IDA resources has heterogeneous effects on regions with an initially lower or higher likelihood of receiving aid.[20] For IDA resources, we draw on the IDA position, which is the "Bank‘s net investment portfolio & its non-negotiable, non-interest-bearing demand obligations (on account of members‘ subscriptions and contributions)" divided by "sum of the Bank‘s undisbursed commitments of development credits and grants" (Dreher et al., 2021b). If there are more funds available, the Bank may exhaust the funds and allocate them to recipient countries. Countries and regions already involved in projects may receive a larger share of the additional funds, partly due to lower costs of information screening and other preparation costs.[21]

Variation in the funding position, defined as "the extent to which IDA can commit to new financing of loans, grants, and guarantees given its financial position" (World Bank, 2015),

[20] The idea is based on Lang (2016) and Gehring and Lang (2020), who employ such a supply-push identification approach using variation in the IMF's liquidity.

[21] Galiani et al. (2017) use Gross National Income (GNI) as a threshold for IDA eligibility. We prefer liquidity over graduation for three reasons. First, the continuous liquidity treatment covers a less specific LATE. Only a few countries graduate, and due to exception rules, not all experience reductions of WB aid afterward. Second, Kerner et al. (2017) suggest that countries have leeway to postpone graduation by reporting lower GNI estimates. Our sample finds that the threshold does not always imply a strict reduction of IDA allocations.

can be caused by internal adjustments, shareholders' timing of payments, and repayments by large borrowers like India. It should be exogenous to stability in any individual sub-national African region, particularly conditional on country or even country-year fixed effects.[22]

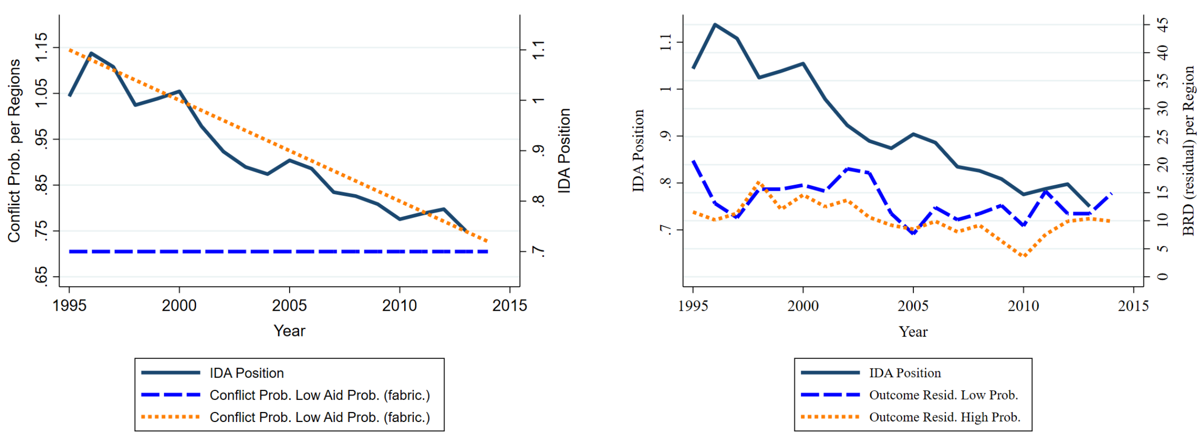
The IDA funding position is obtained by Dreher et al. (2021b) from 1995 to 2007 and the World Bank's annual financial reports from 2008 onwards.[23] This is interacted with the region's pre-determined probability to receive aid, $p_{i,c,t-2}$, to capture that higher probability regions should profit more from higher funding positions. For simplicity, we do not display fixed effects, time trends, and control variables here so that the first stage equation becomes

$$Aid_{i,c,t-1} = \alpha_1 p_{i,c,t-2} + \alpha_2 IDA_{t-1} + \alpha_3 p_{i,c,t-2} IDA_{t-1} + \epsilon_{i,c,t-1} \tag{7}$$

Figure 13: WB's IDA Funding Position and Conflict Outcomes for Low and High Probability Regions

(a) Problematic Trends (Fabricated) in Outcomes

(b) Actual Trends in Outcomes



Notes: Figure (a) displays the temporal variation we use in our interacted instrument, the IDA Funding Position (solid line), along with fabricated trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The trends are fabricated to illustrate potentially problematic trend differences that could induce a spurious correlation. Figure (b) displays the IDA Funding Position (solid line), along with the actual trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The displayed outcomes in (b) are the probability of experiencing a regional conflict of more or equal to five Battle-related Deaths per year.

One potential problem associated with these types of IV approaches highlighted by Christian and Barrett (2017) is that, even if the temporal variation is plausibly exogenous, trends in the time series may overlap with differing trends in the outcome variable, leading to a spurious IV effect. This risk is exacerbated if the time series is relatively short and dominated by long-term trends (Christian and Barrett, 2017). The left-hand side of Figure 13 shows how systematic differences in the long-term conflict trends between low and high probability

[22]One worry is a correlation of the IDA position with the global level of conflict, which would be particularly problematic if there was a differential correlation with conflict in high and in low probability regions. Tables 17 and 18 indicate that controlling for global conflict levels interacted with the probability does not affect the first or second stage results.

[23]Because the WB's fiscal year ends in June, we consider the reported positions in the fiscal years t and t-1 which can both affect disbursements in t-1.

regions could bias estimates. The right-hand side figure shows that the relevant variation in outright conflict exhibits no such trends across high and low probability regions. Despite a general decline in the funding position, there is sufficient year-on-year variation. We take the concerns by Christian and Barrett (2017) serious and provide a large set of robustness tests on the instrumental variable approach in our corresponding working paper (Gehring et al., 2019).

B.2.2 Application to China

Regarding China, we rely on the fact that the economic structure and political incentives frequently lead to excess domestic commodity production. To clear markets and protect domestic companies from potential losses, China commits to more aid projects abroad (Bluhm et al., 2020a; Dreher et al., 2021b). This pattern is not entirely unknown from European agricultural overproduction. These additional projects are often large-scale infrastructure projects that directly use overproduced commodities as inputs (Bräutigam, 2011). However, (Bluhm et al., 2020a) show that commodity (over-)production also induces variation in other sectors like education or health. (Over-)production, thus, captures a local average treatment effect, but seems to trigger variation in the sectors that are overall representative of Chinese aid.

Chinese "mega-deals" (Strange et al., 2017) cannot easily be duplicated or scaled within regions. Moreover, the donor tries to expand its influence during our sample period. Thus, additional projects are more often implemented in low probability regions with no or very few projects.

We take the measure of Chinese domestic commodity over-production, $T_{i,c,t}$ from (Bluhm et al., 2020a). China's production of aluminum, cement, iron, and steel is measured in 10,000 tons, glass in 10,000 weight cases, and timber in 10,000 cubic meters. As there clearly is an overall upward trend in production over time, they detrend the individual time series. Principal component analysis is used to extract the first common factor from these inputs, resulting in one variable that maximizes the variation of the underlying components.

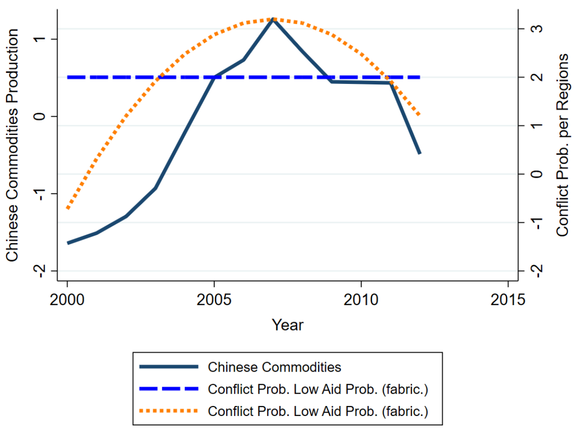
This time-varying variable $T_{i,c,t}$ is interacted with the region's pre-determined probability to receive aid, $p_{i,c,t-3}$. The intuition is that lower probability regions should profit more from Chinese commodity overproduction since China is still expanding its project portfolio to new regions when feasible. Hence, the exogenous variation over time stems from the Chinese time series, while the regional probability is considered potentially endogenous but pre-determined. The first stage equation is

$$Aid_{i,c,t-2} = \alpha_1 p_{i,c,t-3} + \alpha_2 Commodity_{t-3} + \alpha_3 p_{i,c,t-3} Commodity_{t-3} + X^{Ex}_{i,c,t}\alpha_4 + \epsilon_{i,c,t-2} \quad (8)$$

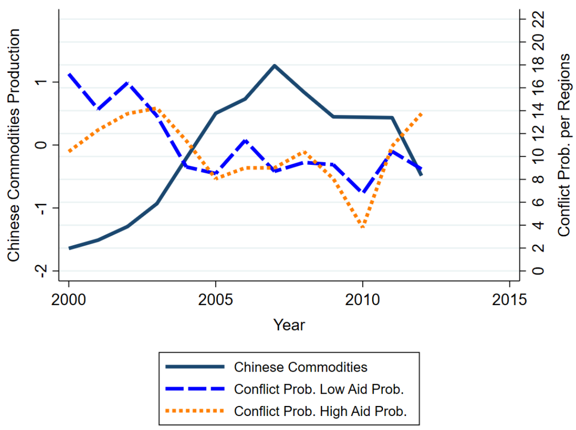
The left-hand side of Figure 14 illustrates differing long-term conflict trends in low and high probability regions, which would lead to biased estimates. The commodity time series variable is inverse U-shaped. The IV results may be spurious if conflict trends in either low or high probability regions would, for other reasons, also follow such a pattern. The right-hand side graph, however, assures us that this does not seem to be the case. Since the Chinese time series has a stronger trend component than the WB, we will investigate the robustness of this specification with additional tests.

Figure 14: Chinese Commodities Production and Conflict Outcomes for Low and High Probability Regions.

(a) Problematic Trends (fabricated) in Outcomes



(b) Actual Trends in Outcomes

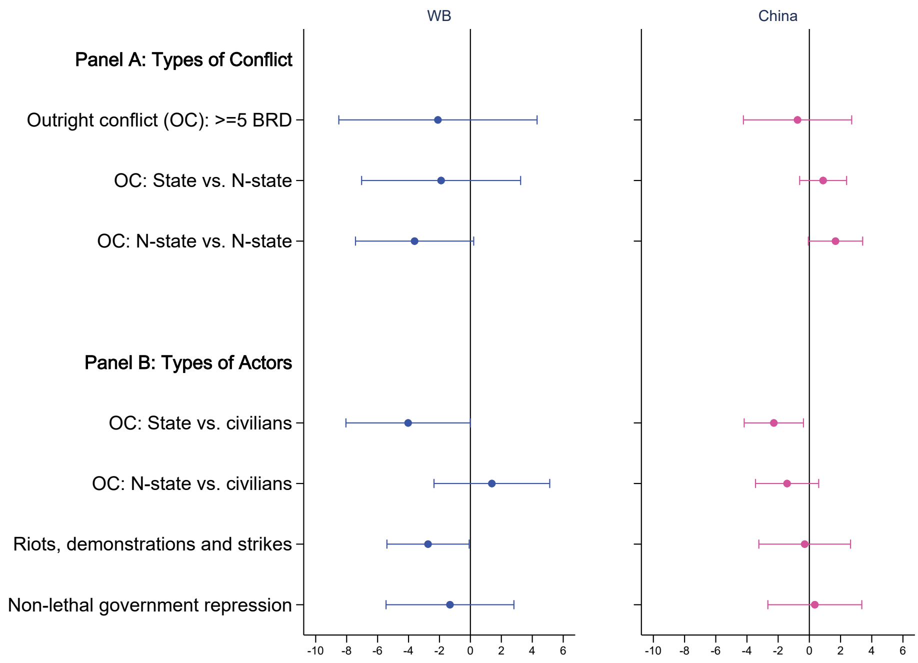


Notes: Figure (a) displays the temporal variation we use in our interacted instrument, the Chinese Commodities Production (solid line), along with fabricated trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The fabricated trends illustrate potentially problematic trend differences that could induce a spurious correlation. Figure (b) displays the Chinese commodity (over-)production (solid line), along with the actual trends in the conflict outcomes for low (long-dashed line) and high probability (short-dashed line) recipient regions. The displayed outcomes in (b) are the probability of experiencing a regional conflict of more or equal to five Battle-related Deaths per year.

B.3 IV Results

Figure 15 shows the main results of Figure 4 replicated with the instrumental variable approach. We focus on the stricter specifications using country-year FE for this figure and the following robustness tests. For the WB, the estimates are, on average, more negative than for China, as with the FE specifications. However, due to the large confidence intervals, they are mostly insignificant. For China, we again find a robust null effect. Overall, the patterns are similar to the ones with our main FE specification. However, the estimates are much noisier than the FE estimates. This leads to very large confidence intervals and mostly insignificant coefficients. It is hard to assess if those large intervals are due to insufficient variation in the time series component of the instrument or an appropriate depiction of real uncertainty.

Figure 15: Coefficient Plot – Instrumental Variable



Notes: The figure shows coefficient plots of individual IV regressions. The respective outcome variables – outright conflict, protest, and government repression incidence indicators – taking on the value 100 if there was at least one event in the respective category, are regressed on the standard deviation of $ln(aid + 0.01\,\mathrm{USD})$. The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Events are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Exogenous (time-varying) controls and country-year FE are included in all regressions. Time trends included consist of linear and squared country-specific time trends as well as linear regional time trends. 90% confidence intervals are based on standard errors, clustered two-way at the country-year and regional levels. Full results displayed in table 13.

While the transparent and straightforward FE approach avoid these issues, it is reassuring to see that applying an IV approach that is most popular in the aid literature yields no sign of a systematic conflict-fueling effect.

Nonetheless, an important piece of literature, most prominently Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2018), has identified potential issues with the Bartik and shift-share approaches. While usually, only the time series is exogenous in a strict sense, the studies show that often the cross-sectional dimension contributes to identification in an important way. A central criticism of those papers is, thus, the lack of transparent reporting of the importance and weights of different industries/countries of origin on the first stage, compared to the influence of the time series. Given those issues with this IV approach, the next section systematically evaluates potential concerns about the approach in general and for China specifically.

B.4 Sensitivity of IV estimates

As described above, some of the issues from the literature on Bartik and shift-share approaches (e.g., Goldsmith-Pinkham et al., 2020; Borusyak et al., 2018) also apply to the instrumental variable approach taken here. The main concerns can roughly be summarized as:

1. What type of variation is there in the first stage (how many and which units are moved by the instrument)? Is there sufficient variation?
2. To what extent is the first stage driven by the potentially endogenous cross-sectional variable? Which units and observations matter most?
3. Is there enough variation in the time series part? Or is it dominated by long-term trends that could create a spurious correlation?

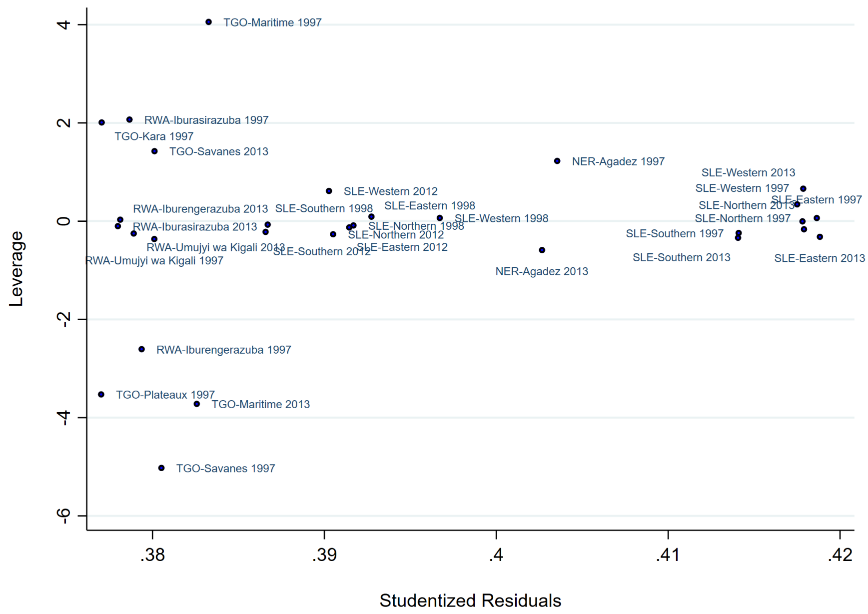
We structure our robustness tests according to those concerns, adapting the common tests in the related literature and adding some additional checks. Specifically, we check for leverage of the probability of individual regions on the first stage, resembling Table 1 and Figure 1 in Goldsmith-Pinkham et al. (2020). We will also run leave-one-out specifications, resembling the common approach in the related literature. Moreover, we engage in many other sensitivity tests, including a placebo analysis, to show that spurious correlations do not drive our first stages.

B.4.1 Variation Induced by Instrument

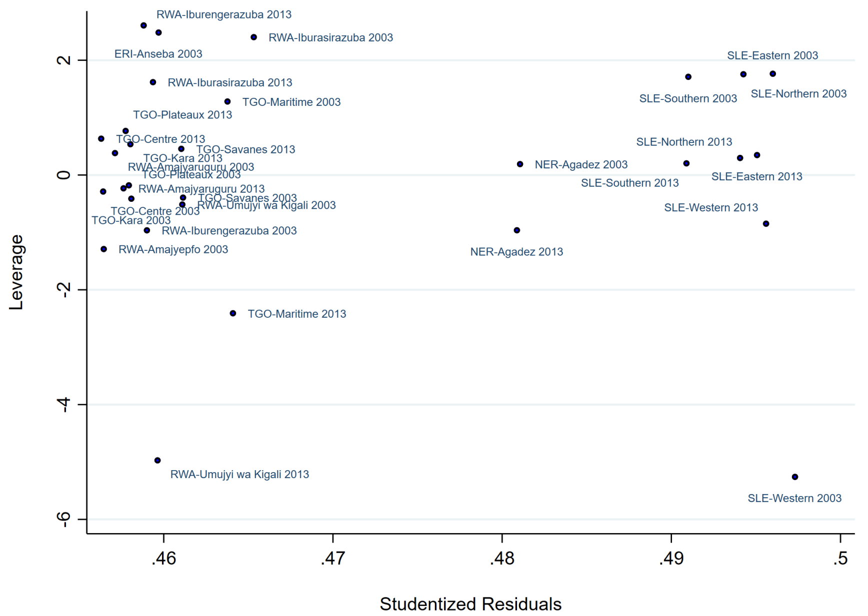
Goldsmith-Pinkham et al. caution particularly against the fact that a small fraction of cross-sectional units is driving the first stage, which would increase the risk of a spurious correlation. To address this concern in our setting, Figure 16 identifies for each donor the 30 region-years with the highest leverage.

Figure 16: Top30 Regions With Highest Leverage

(a) 30 Regions With Highest Leverage for World Bank IV (ADM1)



(b) 30 Regions With Highest Leverage for Chinese IV (ADM1)



Notes: In order to visually investigate the presence of outliers and influential observations, the graphs plot the Top30 leverages - the diagonal elements of the projection matrix -, against studentized (jackknifed) residuals, i.e., the leave-one-out residuals divided by their estimated standard deviation. Some leverages do exceed the rule of thumb $(2k+2)/n$, where k is the number of predictors and n is the number of observations. This threshold translates in $(2133 \times 2+2)/12376 = 0.345$ for WB and $(1908 \times 2+2)/8736 = 0.437$ for China. It is reassuring, though, that the absolute values of studentized residuals are far from exceeding the rule of thumb 2. For both China and WB, the countries with the highest leverage observations are TGO (Togo), SLE (Sierra Leone), RWA (Rwanda), ERI (Eritrea), and NER (Niger).

Figures 16a and 16b show that the high leverage region-years for both IV approaches are located in Niger, Rwanda, Sierra Leone and Togo). Table 14 provides IV estimates without those Top30 leverage regions, and as a stricter test also without the top 1% and top 5% region-years with the highest leverage. The Top30 already contains all observations that would be considered outliers following common rules-of-thumb. Also excluding these high leverage observations, the second stages never show a positive and significant conflict-fueling effect.

Moreover, we implement a jackknife (leave-one-out) test, omitting each country once when estimating the first stage of the IV. Figures 18 and 19 show the results. The estimates indicate that the first stage coefficients are very stable for both the WB and China. Hence, we find no evidence that the first stage is sensitive to variations in the cross-sectional term induced by specific regions or whole countries.

B.4.2 Role of the Cross-sectional Terms

Remember that the instrument resembles

$$Aid_{i,t} = p_{i,t-1} \times TimeSeries_t, \tag{9}$$

with different time series and lag structures applied to the WB and China. A first major concern is the importance of the cross-sectional component. Goldsmith-Pinkham et al. (2020) explicitly discusses concerns that the endogenous cross-sectional shares drive the IV results rather than the exogenous time series. We address this concern in several ways. First, unlike most of the literature, our main specification already uses a predetermined probability that contains only data prior to the aid commitment or disbursement decision. In addition, we test the sensitivity by imposing a stronger assumption regarding predetermination. Instead of lagging the probability term by one year, we define an alternative interacted instrument based on an initial probability from the first three sample years (1995 to 1997 for the WB's IDA; 2000 to 2002 for Chinese Commodities) in Table 16.

Moreover, we assess the risk that the cross-sectional component alone is driving the first stage results. Christian and Barrett (2017) suggest several steps to assess the sensitivity of such IV approaches to spurious time trends. As a first illustrative step, Figures 13 and 14 provided a visual inspection of the identifying time series and the outcome among infrequent and frequent aid recipients. The figures indicate that the instrumental variables (IDA budget and Chinese commodities) do not follow the same trend as the outcome variable, neither for infrequent nor for frequent aid recipients.[24]

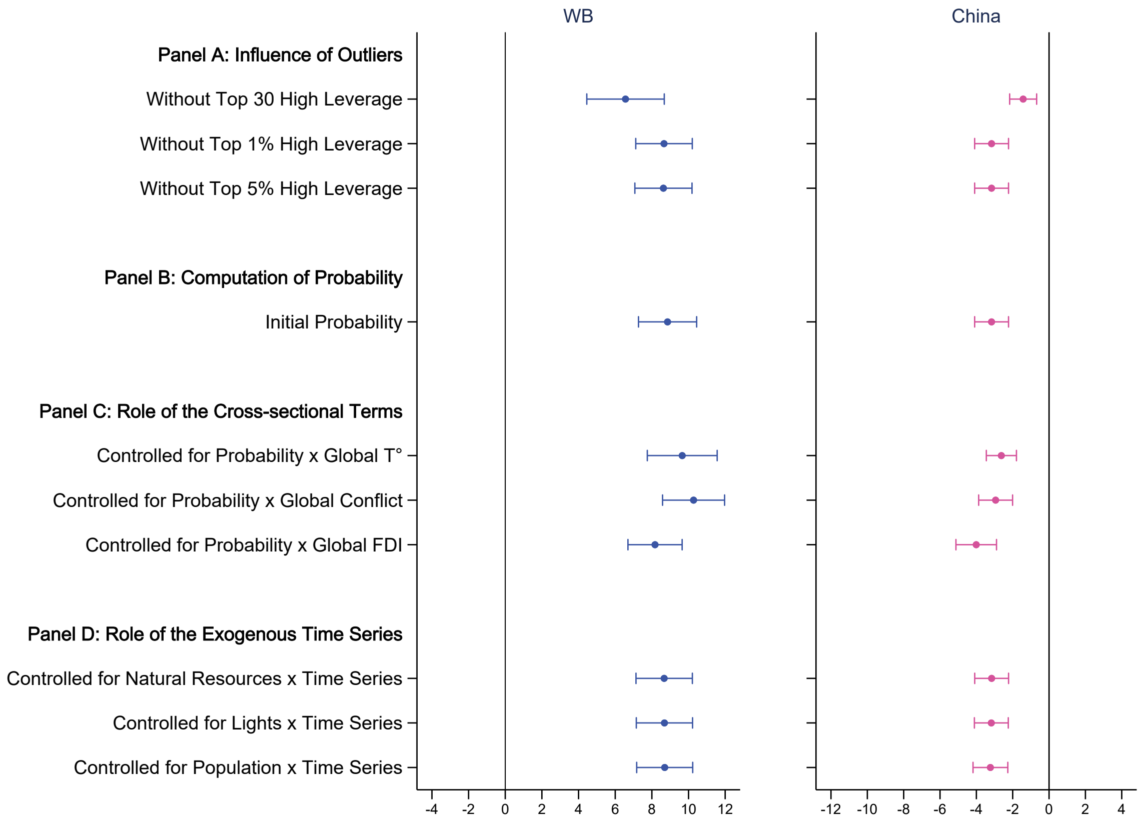
Moreover, we test the sensitivity towards an overlap with other global trends by interacting the regional probability part of the instrument with alternative placebo global time series. Specifically, we use global temperature, global conflict, and global FDI to cover important trends in different dimensions. The global temperature could be linked to weather shocks and food prices – issues often associated with conflict in the literature (Miguel et al.,

[24] While the latter two outcomes series for infrequent and frequent aid recipients apparently follow a similar trend for China, this is in line with the null result, which we find in our main analysis.

2004; Berman and Couttenier, 2015). Global conflict might obviously correlate with regional conflict, and global FDI is proposed as a proxy for the effect of global economic conditions by Bluhm et al. (2020b).

Figure 17, panel C, shows first stage estimates when controlling for the interaction of the actual regional probability term with those three time series. For both the WB and China, the first stage estimates are not affected. They remain of similar size and are statistically highly significant. Moreover, as Tables 17 and 18 show, the second stage estimates never suggest a positive, conflict-fueling effect.

Figure 17: Coefficient Plot First Stages – Instrumental Variable



Notes: The figure shows coefficient plots of individual first stage regressions. Exogenous (time-varying) controls and country-year FE are included in all regressions. Time trends included consist of linear and squared country-specific time trends as well as linear regional time trends. 90% confidence intervals are based on standard errors, clustered two-way at the country-year and regional levels. Full results are displayed in table 15 for panel A, table 16 for panel B and tables 17, 18 for panels C and D.

To further assess the possibility that spurious correlations alone would be causing a strong first stage to drive results, we conduct a randomization test regarding the time series dimensions. Specifically, we randomize across the time series' years and then use this randomized time series to compute the instrument. Figures 21a and 21b show that when using a random version of the time series, the interaction with the cross-sectional IV term does not generate a significant first stage. The estimates are symmetrically centered around zero. In terms of size, they are considerably smaller in magnitude than our real first stage coefficients. None of the second-stage estimates suggest a positive, conflict-fueling effect.

B.4.3 Role of the Exogenous Time Series

Another challenge to identification highlighted by Christian and Barrett (2017) is that those spurious correlations in regional variables might drive the explanatory power of the regional probability term rather than the likelihood to receive aid. Mirroring the approach in the section before, we also test the extent to which the first stage might be driven by the time series part alone. To do so, we compute additional placebo instruments by interacting the actual time series with placebo regional variables. Specifically, we use a region‘s endowment with resources, economic development (measured using nighttime light), and population size. All factors might relate to other underlying reasons why a region might receive more or less aid. Adding them as interactions with the time series to our regressions can inform us whether the instrument relies too much on the information contained in the time series.

Figure 17, panel D, shows the first stage estimates when controlling for those terms. Again, the first stage estimates are not affected for both the WB and China. They remain of similar size and are statistically highly significant. Moreover, as Tables 17 and 18 show, the second stage estimates never suggest a positive, conflict-fueling effect.

Finally, analogous to Figures 21a and 21b, we conduct a randomization test regarding the cross-sectional dimension of our instrument. This time, we randomize across the country-regions of the regional probability part and then use these randomized probabilities to compute the instrument. Figures 20a and 20b show again that when using a random version of the time series, the interaction with the cross-sectional IV term does, on average, not generate significant first stage estimates, which are also considerably smaller in magnitude than our real first stage coefficients.

Specific Worries about the Time Series used for the Chinese Aid First Stage Finally, we implement a set of tests to address some worries with respect to spurious trends that are specific to the Chinese instrument and the time series that is part of it. First, we use the Chinese commodity time series without detrending. Second, we use a Chinese steel production time series instead of the full spectrum of commodity prices. Finally, we use US steel production as a placebo test to demonstrate that we are not just picking up a spurious trend but also a China-specific commodity shock. Table 11 shows in columns 2 and 3 that the alternative Chinese time series also lead to a relevant first stage, with coefficients in the same direction and is statistically highly significant. Those significant findings reassure us that the first stage does not pick up a trend of one particular time series. When using US steel as a placebo, instead, the coefficient is close to zero and insignificant. Hence, while acknowledging that there is certainly a trend component in the Chinese time series, particularly the robustness to the placebo exercise is reassuring.

Table 11: China - Further Checks: Variation of IV Time Series Component

Time series	*Detrended Commodities* (Baseline)	*Non Detrended Commodities*	*Ch Steel Production*	*US Steel Production* (Placebo)
IV Second stage				
$\ln(Chinese\ Aid_{\text{t-2}})$	-0.757	0.385	-1.717	-159.620
	(2.110)	(1.013)	(3.239)	(2517.602)
KP p-value	0.000	0.000	0.000	0.946
KP F-stat	31.190	74.289	16.456	0.004
IV First stage				
$Time\ Series_{\text{t-3}} \times \text{P}_{\text{t-3}}$	-3.1626***	-11.2274***	-15.1094***	-0.0016
	(0.5664)	(1.3027)	(3.7259)	(0.0246)
N	7975	7975	7975	7975

Notes: The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year, country, country-year FE, as well as time trends. Time trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

To summarize, the IV results have much larger confidence intervals than the FE estimates, highlighting a potential issue with the lack of variation along the time dimension in the instruments. However, applying all those sensitivity analyses, the IV remains relevant, and we find no specification with a significant positive effect of aid on conflict. This holds for both China and the World Bank. Acknowledging all the limitations of the approach, the results of the commonly used IV approach (Nunn and Qian, 2014; Dreher et al., 2021b) strengthens our confidence further that neither WB nor Chinese aid stimulates conflict in African regions.

B.5 IV: Additional Figures and Full Regression Results

Table 12: Full IV Main Results - Aid and Conflict ADM1-Level

Panel A: World Bank Aid	
IV Second Stage: World Bank	
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-2.0983
	(3.9060)
Kleibergen-Paap underidentification test p-value	0.000
Kleibergen-Paap weak identification F-statistic	86.724
IV First stage: World Bank	
$IDA\,Position_{\text{t-1}} \times \text{P}_{\text{t-2}}$	8.6799***
	(0.9321)
N	12325
Panel B: Chinese Aid	
IV Second Stage: China	
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.7571
	(2.1101)
Kleibergen-Paap underidentification test p-value	0.000
Kleibergen-Paap weak identification F-statistic	31.190
IV First Stage: China	
$Chinese\,Commodity_{\text{t-3}} \times \text{P}_{\text{t-3}}$	-3.1626***
	(0.5663)
N	7975

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD<5). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors are in parentheses, two-way clustered at the country-year and regional level. The specification is identical to Table 3, column (8). Please note that the first stage results are not provided in standard deviations. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 13: Full IV – Conflict Actors and Types ADM1-Level

	OC: BRD≥5	OC: State vs. N-State	OC: N-State vs. N-State	OC: State vs. Civilians	OC: N-State vs. Civilians	Riots, Demonstr., Strikes	Non-lethal Gvt. Repression
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: WB Aid							
IV Second Stage: WB							
(World Bank Aid $_{t\text{-}1}$)	-0.2089	-0.4563*	0.1896	-0.2666	-0.1753	-0.2467	-0.3683
	(0.4236)	(0.2668)	(0.2453)	(0.1757)	(0.2789)	(0.3359)	(0.2490)
N	12376	12376	12376	12376	12376	12376	12376
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	83.274	83.274	83.274	83.274	83.274	83.274	83.274
Panel B: Chinese Aid							
IV Second Stage: China							
(Chinese Aid $_{t\text{-}2}$)	-0.3790	0.0784	0.2898	-0.6432*	-0.4001	-0.2036	-0.1417
	(0.6224)	(0.2163)	(0.2668)	(0.3315)	(0.3752)	(0.4772)	(0.4706)
N	8736	8736	8736	8736	8736	8736	8736
Kleibergen-Paap underidentification test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap weak identification F-statistic	25.883	25.883	25.883	25.883	25.883	25.883	25.883

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD<5 for Outright Conflict (OC), and 100 if there was at least one event in the respective category of Riots, Demonstrations and Strikes, or Non-lethal Government Repression). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results consider different conflict outcomes, based on specification (8) of Table 3. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions and country-years, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: Based on the literature on development aid and conflict, one may expect that aid may impose different incentives across different actors and, hence, has heterogeneous effects. To address this question, we run analysis for those different outcomes. We find that aid, if anything, is significantly negatively related to conflict. However, there is a fair bit of heterogeneity, suggesting room for future research to explore these results in more detail.

Table 14: ADM1 IV World Bank - Without High Leverage Regions

Without Top	30	1%	5%
IV Second Stage			
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-1.9529	-2.7798	-2.5358
	(3.9604)	(4.0187)	(3.9708)
KP p-value	0.000	0.000	0.000
KP F-stat	85.385	82.912	84.350
IV First stage			
$IDA\,Pos_{\text{t-1}} \times \text{P}_{\text{t-2}}$	8.6632***	8.6305***	8.8566***
	(0.9375)	(0.9478)	(0.9643)
N	12294	12199	11709

Table 15: ADM1 IV China - Without High Leverage Regions

Without Top	30	1%	5%
IV Second Stage			
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.7140	-0.5073	-1.2534
	(2.1496)	(2.1600)	(2.5699)
KP p-value	0.000	0.000	0.000
KP F-stat	30.147	29.782	27.266
IV First stage			
$Comm_{\text{t-3}} \times \text{P}_{\text{t-3}}$	-3.1508***	-3.1401***	-3.0875***
	(0.5739)	(0.5754)	(0.5913)
N	7944	7896	7577

Notes: The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors are in parentheses, two-way clustered at the country-year and regional level. The specification is identical to Table 3, column (8). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Interpretation: One concern is that the predictive power of the instrumental variables is driven by a few regions that received a lot of aid in the past. The table shows that our results still hold when dropping the Top30, Top1%, Top5% region-year observations with the highest statistical leverage.

Table 16: IV Results - Initial Probability

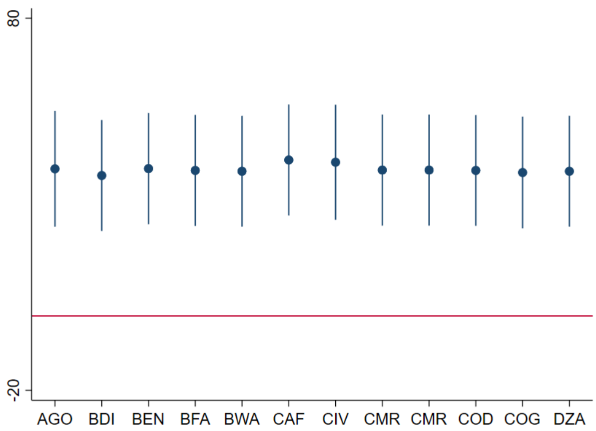
Panel A: World Bank Aid	
IV Second Stage: World Bank	
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-3.1577
	(5.7827)
Kleibergen-Paap underidentification test p-value	0.000
Kleibergen-Paap weak identification F-statistic	26.027
IV First stage: World Bank	
$IDA\,Position_{\text{t-1}} \times \text{P}_{98}$	6.5627***
	(1.2853)
N	11600
Panel B: Chinese Aid	
IV Second Stage: China	
$\ln(Chinese\,Aid_{\text{t-2}})$	-5.1572
	(5.9105)
Kleibergen-Paap underidentification test p-value	0.002
Kleibergen-Paap weak identification F-statistic	9.925
IV First stage: China	
$Chinese\,Commodity_{\text{t-3}} \times \text{P}_{03}$	-1.4250***
	(0.4516)
N	7250

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors are in parentheses, two-way clustered at the country-year and regional level. The specification is identical to Table 3, column (8). Please note that the first stage results are not provided in standard deviations. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

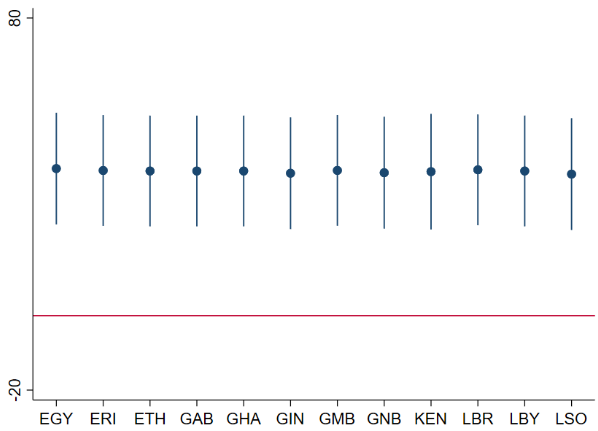
Interpretation: One concern is that the IV is driven by more recent years. Considering only the first three years of the panel to estimate the probability (e.g., considering initial conditions), the main result of no conflict-fueling effect is confirmed.

Figure 18: Robustness of First Stage for World Bank Aid - Leave-out-country-by-country

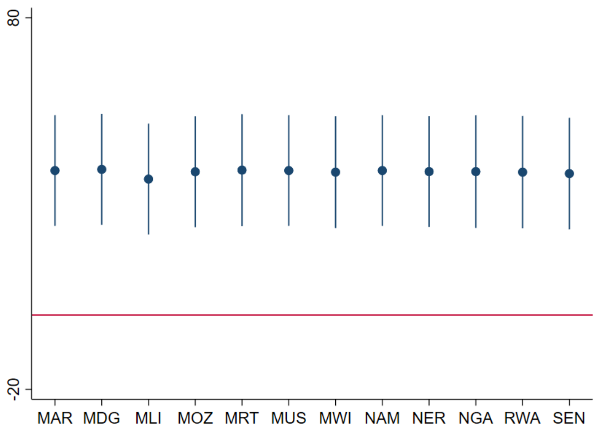
(a) Leave One Out (i)



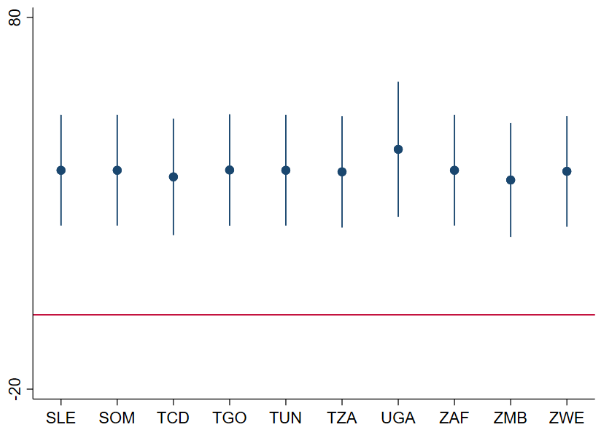
(b) Leave One Out (ii)



(c) Leave One Out (iii)



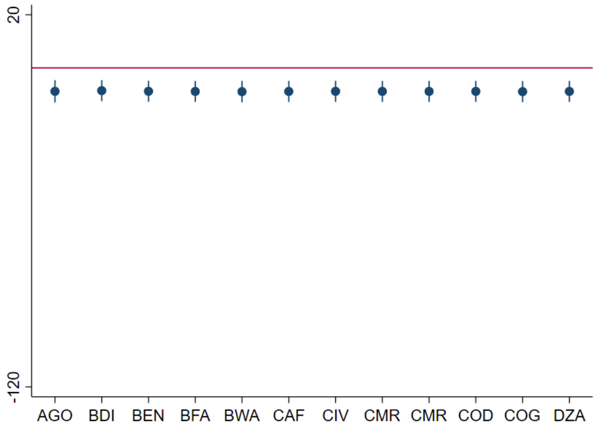
(d) Leave One Out (iv)



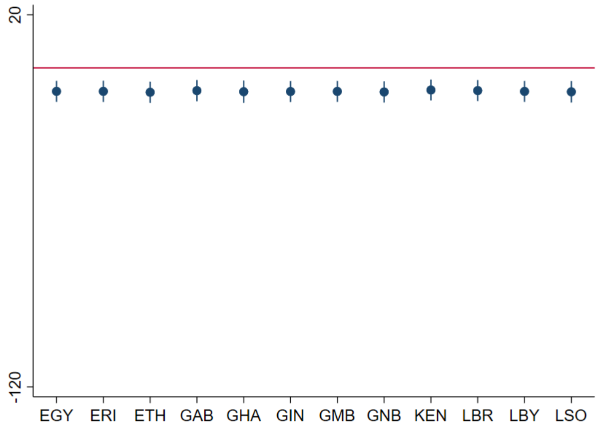
Notes: Results depict coefficients of the instrumental variable $probability_{i,c,t-2} \times IDAPosition_{t-1}$ for different regressions leaving one country out from the estimation. Labels in the graph refer to ISO codes of recipients.
Interpretation: To rule out that one particular country drives the results, we run a series of regressions, each leaving out one country. The graph shows that the results are not affected when doing that for any particular country.

Figure 19: Robustness of First Stage for Chinese Aid - Leave-out-country-by-country

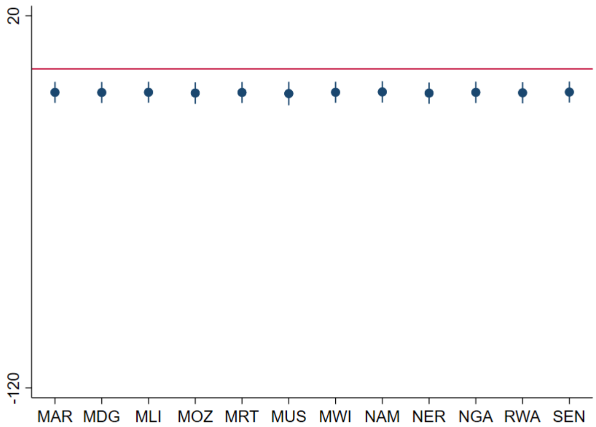
(a) Leave One Out (i)



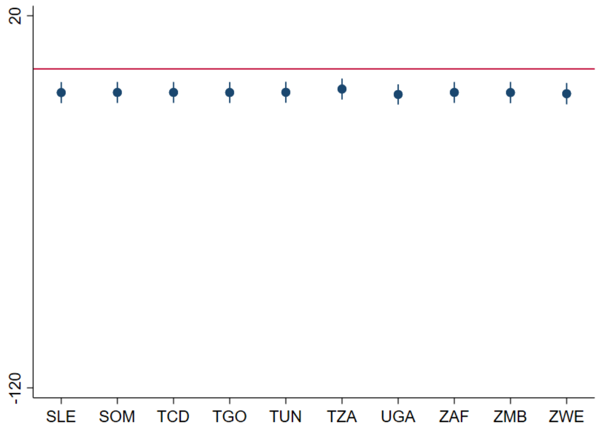
(b) Leave One Out (ii)



(c) Leave One Out (iii)



(d) Leave One Out (iv)



Notes: Results depict coefficients of the instrumental variable $probability_{i,c,t-3} \times ln(Chinese\, Commodity_{t-3})$ for different regressions leaving one country out from the estimation. Labels in the graph refer to ISO codes of recipients.

Interpretation: To rule out that one particular country drives the results, we run a series of regressions, each leaving out one country. The graph shows that the results are not affected when doing that for any particular country.

Table 17: WB with Further Controls - Full Regression Results

Additional control for	$P_{t-2}\times$ Other time series			Other regional variables$\times IDA\,Pos_{t-1}$		
	Global T°	Global conflict	Global FDI	Natural resources	Lights	Population
IV Second stage						
$\ln(World\,Bank\,Aid_{\text{t-1}})$	1.420	1.550	-4.178	2.530	2.580	2.567
	(3.521)	(5.247)	(5.240)	(3.489)	(3.480)	(3.489)
KP p-value	0.000	0.001	0.000	0.000	0.000	0.000
KP F-stat	45.789	22.680	51.444	31.066	30.997	31.048
IV First stage						
$IDA\,Pos_{\text{t-1}} \times \text{Cum. P}_{\text{t-2}}$	9.6564***	10.2754***	8.1731***	8.6705***	8.6799***	8.6990***
	(1.1584)	(1.0286)	(0.8985)	(0.9351)	(0.9326)	(0.9286)
N	12325	12325	12325	12325	12325	12325

Notes: The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors are in parentheses, two-way clustered at the country-year and regional level. Both regressions include year, country, country-year FE, as well as time trends. Time trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Interpretation: One concern is that the predictive power of the instrumental variables is driven by spurious correlations. The table shows that our results still hold when controlling for global trends interacted with regional probability or regional variables interacted with the time series.

Table 18: China with Further Controls - Full Regression Results

Additional control for	$P_{t-3}\times$ Other time series			Other regional variables$\times Commodities_{t-3}$		
	Global T°	Global conflict	Global FDI	Natural resources	Lights	Population
IV Second stage						
$\ln(Chinese\ Aid_{\text{t-2}})$	-1.204	-0.416	-0.803	-0.816	-0.746	-1.072
	(2.708)	(2.388)	(1.713)	(2.116)	(2.103)	(2.086)
KP p-value	0.000	0.000	0.000	0.000	0.000	0.000
KP F-stat	27.149	26.845	34.938	31.180	31.391	31.328
IV First stage						
$Comm_{\text{t-3}} \times \text{Cum. P}_{\text{t-3}}$	-2.6237***	-2.9398***	-4.0079***	-3.1597***	-3.1757***	-3.1834***
	(0.5036)	(0.5675)	(0.6782)	(0.5660)	(0.5669)	(0.5689)
N	7975	7975	7975	7975	7975	7975

Notes: The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Both regressions include year, country, country-year FE, as well as time trends. Time trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Interpretation: One concern is that the predictive power of the instrumental variables is driven by spurious correlations. The table shows that our results still hold when controlling for global trends interacted with regional probability or regional variables interacted with the time series.

Figure 20: Panel A: Randomized Regions Within Years - Probability

(a) WB

(b) China

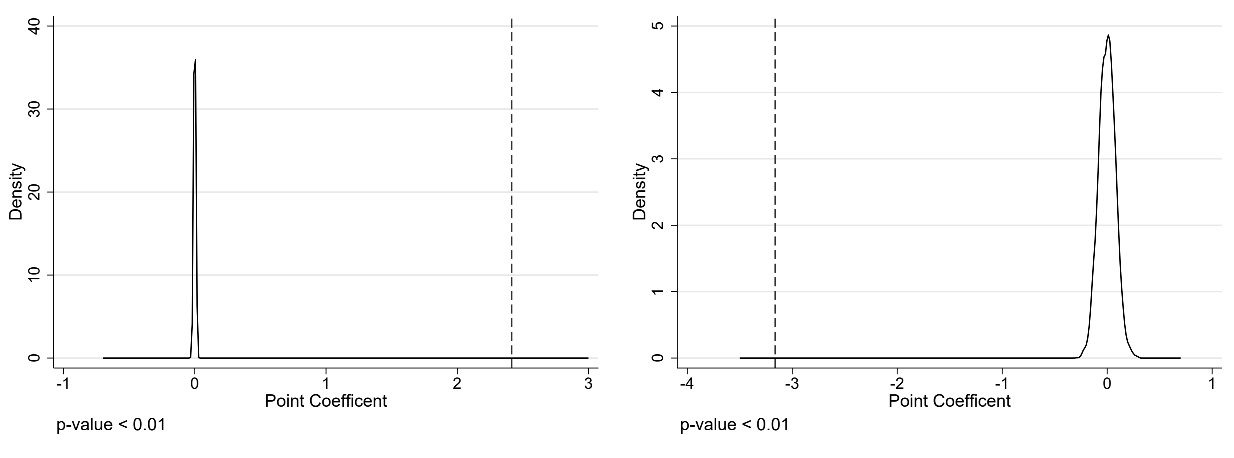
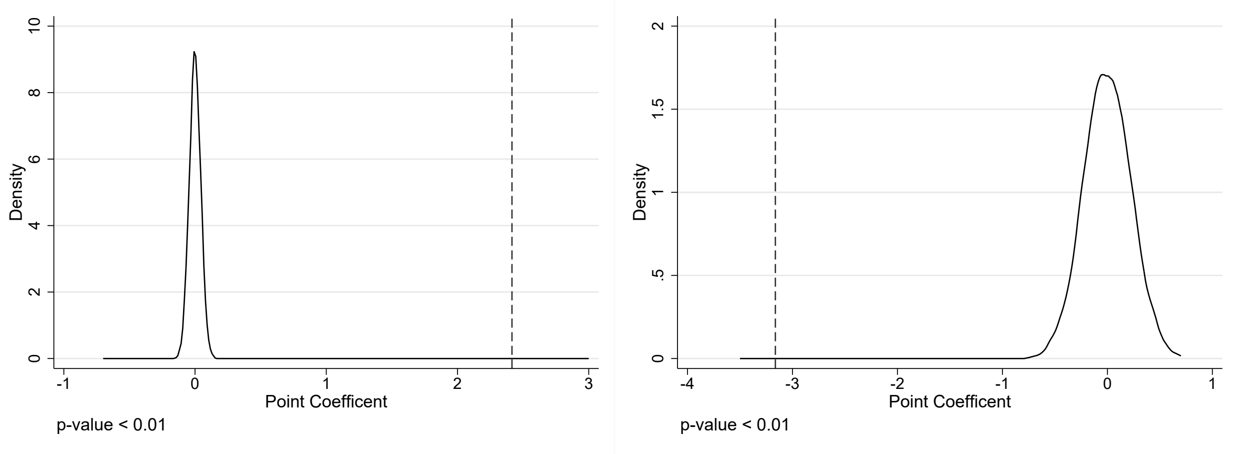


Figure 21: Panel B: Randomized Years Within Regions - Time Series

(a) WB

(b) China



Notes: 5000 placebo treatment assignments of probability and time series were simulated to compute each of the four distributions. As expected, they are concentrated around 0, strengthening the evidence in favor of the causal mechanism we aim to identify. The dashed line represents our estimated first stage.

C Full regression Results for Figures in Main Paper

C.1 Leads and Lags - Full Regression Results

Table 19: ADM1 - Leads and Further Lags

Panel A: WB Aid	(1)	(2)
$\ln(World\,Bank\,Aid_{\text{t+1}})$	-0.0553	1.4529
	(1.1520)	(1.0477)
$\ln(World\,Bank\,Aid_{\text{t}})$	-1.0146	-1.9825**
	(0.9755)	(0.9168)
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.1993	-0.8697
	(0.8321)	(0.8388)
$\ln(World\,Bank\,Aid_{\text{t-2}})$	0.4812	1.3270
	(0.8160)	(0.9463)
$\ln(World\,Bank\,Aid_{\text{t-3}})$	-0.7558	-0.4983
	(0.8182)	(0.9318)
N	10150	10150
Panel B: Chinese Aid	**(1)**	**(2)**
$\ln(Chinese\,Aid_{\text{t+1}})$	0.6748	0.8362*
	(0.4975)	(0.4974)
$\ln(Chinese\,Aid_{\text{t}})$	-0.0509	0.0927
	(0.5070)	(0.5487)
$\ln(Chinese\,Aid_{\text{t-1}})$	-0.0344	-0.1933
	(0.6094)	(0.6270)
$\ln(Chinese\,Aid_{\text{t-2}})$	0.0484	-0.2029
	(0.4643)	(0.5159)
$\ln(Chinese\,Aid_{\text{t-3}})$	0.2296	-0.1238
	(0.3927)	(0.4491)
N	6525	6525
Exogenous Controls	Yes	Yes
Exogenous Controls × Year FE	Yes	Yes
Linear Regional Trends	Yes	Yes
Country × Year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD<5). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for China from 2002 to 2014 due to the lag structure. Both regressions include year and region fixed effects as well as linear and squared country-specific time trends. The leads and lags are reported in standard deviations of $ln(aid + 0.01\,\text{USD})$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Click here to go back to section 4.

C.2 Conflict Actors and Types - Full Regression Results

Table 20: FE – Conflict Actors and Types

	OC: State vs. N-State		OC: N-State vs. N-State		OC: State vs. Civilians		OC: N-State vs. Civilians		Riots, Demonstr., Strikes		Non-lethal Gvt. Repression	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: WB Aid												
ln($World\ Bank\ Aid_{\text{t-1}}$)	-1.1449**	-1.2723**	-0.3246	-0.7306	-0.5557	-0.3467	-0.9691**	-0.9125**	-0.0326	-0.0861	-0.2942	-0.7206
	(0.5399)	(0.5718)	(0.3882)	(0.4892)	(0.3477)	(0.3577)	(0.3978)	(0.4400)	(0.7264)	(0.8341)	(0.6061)	(0.7894)
N	13050	13050	13050	13050	13050	13050	13050	13050	13050	13050	13050	13050
Panel B: Chinese Aid												
ln($Chinese\ Aid_{\text{t-2}}$)	-0.0036	-0.1392	-0.0651	0.0063	-0.2820	-0.2509	-0.1358	-0.1340	-0.0732	0.0565	0.1093	-0.0003
	(0.1971)	(0.3534)	(0.2127)	(0.2639)	(0.1717)	(0.1817)	(0.1172)	(0.1491)	(0.4034)	(0.5060)	(0.4018)	(0.4996)
N	8700	8700	8700	8700	8700	8700	8700	8700	8700	8700	8700	8700
Country-year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD≥5, 0 if BRD<5 for Outright Conflict (OC), and 100 if there was at least one event in the respective category of Riots, Demonstrations and Strikes, or Non-lethal Government Repression). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (7) and (8) of Table 3. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: Based on the literature on development aid and conflict, one may expect that aid may impose different incentives across different actors and, hence, has heterogeneous effects. To address this question, we run analyses for those different outcomes. We find that aid, if anything, is significantly negatively related to conflict. However, there is a fair bit of heterogeneity, suggesting room for future research to explore these results in more detail.

C.3 Afrobarometer - Full Regression Results

Table 21: Mechanisms - Afrobarometer

	WB	WB	China	China
Panel A: Security				
Security facilities: Police station present within walking distance?	0.001	0.008*	0.002	-0.004***
	(0.003)	(0.003)	(0.002)	(0.003)
Security forces: Any policemen or police vehicles?	0.002	0.004	0.001	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)
Security forces: Any soldiers or army vehicles?	0.002*	0.005***	-0.001	-0.003
	(0.001)	(0.003)	(0.001)	(0.002)
Frequency of things stolen in past year?	-0.001	-0.006**	0.004*	0.004***
	(0.002)	(0.002)	(0.002)	(0.002)
Frequency of phsysical attacks in the past year?	-0.000	-0.003***	0.001	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)
Panel B: Democratic norms and attitudes				
Democracy: How democratic is your country today?	-0.002	0.003	-0.005*	-0.000
	(0.002)	(0.003)	(0.002)	(0.003)
Democracy: Did you perceive last elections as free and fair?	-0.003	-0.003	-0.012**	-0.012
	(0.005)	(0.007)	(0.004)	(0.008)
Governance: Reject one-party rule	0.003	0.013*	-0.006	-0.003
	(0.005)	(0.005)	(0.004)	(0.006)
Governance: Reject military rule	0.006*	0.008*	-0.002	-0.001
	(0.003)	(0.004)	(0.003)	(0.004)
Governance: Reject one-man rule	0.004*	0.006*	-0.005*	-0.005***
	(0.002)	(0.003)	(0.002)	(0.003)
Reject government banning organizations that go against its policies	0.005*	0.014**	-0.003	0.002
	(0.002)	(0.005)	(0.003)	(0.004)
Panel C: Government responsiveness and repression				
Frequency of contact to government official to express your view	0.003*	0.003***	-0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)
Fear of political intimidation or violence during campaigns	-0.001	-0.008***	0.003	0.011**
	(0.003)	(0.004)	(0.003)	(0.003)
How often do people have to be careful about what they say in politics?	0.000	-0.005	0.002	-0.002
	(0.002)	(0.004)	(0.002)	(0.003)
Rule of Law: People must obey the law	-0.004*	-0.001	0.004**	0.007**
	(0.002)	(0.003)	(0.001)	(0.002)
Country FE	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: The figure shows coefficient plots along with 90% confidence intervals of individual OLS regressions of the respective questions from Afrobarometer on the standard deviation of $ln(aid + 0.01\,\text{USD})$. All outcome measures were standardized, setting the mean to zero. Respondents were matched to the ADM1 regions using the provided geocoordinates. Afrobarometer surveys were conducted in the years 1999-2015 for a varying number of 12 to 36 countries, resulting in an unbalanced panel with uneven gaps between years. Significance levels: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

C.4 Sensitivity of Main FE Results - Full Regression Results

Table 22: FE – Outcome and Treatment Measurement

	OC: $BRD \geq 5$		OC: $BRD \geq 25$		OC: asinh(brd)		Pop.-weighted aid		asinh(aid)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: WB Aid										
ln($World\,Bank\,Aid_{\text{t-1}}$)	-1.4983**	-1.6516**	-1.4243**	-1.2919**	-0.0851***	-0.0839**	-1.5298**	-1.7160**	-1.5939**	-1.7568**
	(0.6684)	(0.7583)	(0.5560)	(0.6252)	(0.0324)	(0.0349)	(0.6730)	(0.7640)	(0.6827)	(0.7705)
N	13050	13050	13050	13050	13050	13050	13050	13050	13050	13050
Panel B: Chinese Aid										
ln($Chinese\,Aid_{\text{t-2}}$)	-0.2624	-0.1392	0.0235	-0.0089	-0.0028	0.0031	-0.2772	-0.1555	-0.2646	-0.1488
	(0.2915)	(0.3534)	(0.2093)	(0.2262)	(0.0116)	(0.0137)	(0.2900)	(0.3539)	(0.2917)	(0.3542)
N	8700	8700	8700	8700	8700	8700	8700	8700	8700	8700
Country-year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator for Outright Conflict (OC) (100 if BRD≥5, 0 if BRD<5 in columns (1) and (2) and 100 if BRD≥25, 0 if BRD<25 in columns (3) and (4)). In columns (5) and (6), we use the inverse hyperbolic sine transformation of the number of Battle-related Deaths. The following columns return to the outcome measure from columns (1) and (2) but change the treatment to population-weighted aid in columns (7) and (8) and the inverse hyperbolic sine transformation of location-weighted aid in columns (9) and (10). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (6) and (8) of Table 3. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: The literature on development aid and its effects indicates that results are oftentimes sensitive to the measurement of dependent and independent variables. To address this issue, we apply different thresholds and transformations. We find that aid if anything, is significantly negatively related to conflict for the WB, and there is an insignificant effect for China.

Table 23: FE – Selection Concerns

	Control for Press Freedom		Control for Pre-Trends		Lagged Dep. Variable		Contrl for other Select. Concerns		Both Donors	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: WB Aid										
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.9628	-1.9434*	-0.9923	-1.1004	-1.3981**	-1.6365**	-1.2693*	-1.5435*	0.6155	-0.7393
	(0.9204)	(1.0116)	(0.7116)	(0.7160)	(0.6474)	(0.7361)	(0.7606)	(0.8257)	(0.8066)	(0.8209)
N	12325	12325	12325	12325	13050	13050	12223	12223	8700	8700
Panel B: Chinese Aid										
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.2497	-0.2168	-0.3192	-0.1624	-0.2676	-0.1482	-0.2642	-0.1488	-0.2635	-0.1384
	(0.4172)	(0.4660)	(0.2988)	(0.3563)	(0.2914)	(0.3568)	(0.2974)	(0.3648)	(0.2918)	(0.3537)
N	8700	8700	8700	8700	8700	8700	8628	8628	8700	8700
Country-year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Columns 9 and 10 focus on the overlapping sample for both donors. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (7) and (8) of Table 3. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Interpretation: This table considers various selection concerns. First columns (1) and (2) control for an interaction of region fixed effects with press freedom (Freedom House, 2021) to account for a potential downward bias in the Chinese aid data due to differential media presence in fragile regions. The neutral effect remains unchanged. Moreover, we account for the fact that donors may select/avoid conflict-prone regions (pre-trends and lagged dependent variable in columns (3)-(6)) and politically/economically more powerful regions (controlling for nightlights, political exclusion, and leader birth regions in columns (7) and (8)). Results indicate a robust neutral to negative effect for the WB and a non-significant effect for China. The only notable change occurs where WB aid switches signs when we include both donors in one regression (columns (9) and (10)). However, the coefficient remains insignificant and turns once more negative with country-year FE, suggesting that it is not controlling for Chinese aid, but the specific years of the overlapping panel which drive this change.

Table 24: FE – Different Clustering Approaches

	Clustering at Regional Level		Clustering at Country-Year Level		Clustering at Country Level	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: WB Aid						
ln($World\,Bank\,Aid_{\text{t-1}}$)	-1.4983**	-1.6516**	-1.4983***	-1.6516***	-1.4983**	-1.6516*
	(0.6258)	(0.7444)	(0.5550)	(0.6111)	(0.5587)	(0.8385)
N	13050	13050	13050	13050	13050	13050
Panel B: Chinese Aid						
ln($Chinese\,Aid_{\text{t-2}}$)	-0.2624	-0.1392	-0.2624	-0.1392	-0.2624	-0.1392
	(0.2729)	(0.2980)	(0.2858)	(0.3154)	(0.3192)	(0.3677)
N	8700	8700	8700	8700	8700	8700
Country-year FE	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (7) and (8) of Table 3. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Interpretation: Ex ante, it is not clear at which level errors would be related (across spatial or temporal dimensions), and researchers have several degrees of freedom in this regard. We also present three other commonly applied levels of clustering, which do not change the significant negative correlation for the WB and the insignificant findings for Chinese aid.

Table 25: FE – Modifiable Area Unit Problem – ADM2

	(1)	(2)
Panel A: World Bank Aid		
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.4697**	-0.5074**
	(0.1867)	(0.1988)
N	105214	105214
Panel B: Chinese Aid		
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.0943	-0.1151
	(0.0732)	(0.1006)
N	70132	70132
Country-year FE	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The sample includes second-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (6) and (8) of Table 3. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time Trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Interpretation: In line with coefficients at the ADM1 level, the more fine-grained classification at the ADM2 level leaves the negative to neutral aid effects unchanged.

Table 26: Negative Binomial and Poisson

	Neg. Binomial	PPML
Panel A: WB Aid		
$\ln(World\,Bank\,Aid_{\,\text{t-1}})$	-0.0530	-0.0330
	(0.0389)	(0.0363)
N	3835	3835
Panel A: Chinese Aid		
$\ln(Chinese\,Aid_{\,\text{t-2}})$	-0.0656	-0.0513*
	(0.0406)	(0.0306)
N	3783	3783

Notes: The dependent variable is a binary conflict indicator (100 if BRD≥5, 0 if BRD<5). Standard errors are in parentheses, clustered at the regional level. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. The sample includes African countries for the sampling period of 1995-2012 for the WB and 2000-2012 for Chinese Aid. All regressions include year fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Click here to go back to section 5.4.
Interpretation: To address concerns that OLS regression is not an optimal method for binary dependent variables and dependent variables with many zeros, we verify our results with Poisson (Silva and Tenreyro, 2011) and a negative binomial estimation. The results are in line with our main specification since the majority of the coefficients are not statistically significant or statistically significant but negative.

Table 27: FE - Heterogeneity – 1/2

	Frac. $\leq$ Med.		Frac. $\geq$ Med.		Coalition: excld.		Coalition: incld.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: WB Aid								
ln($World\,Bank\,Aid_{\text{t-1}}$)	-2.3774***	-1.8937*	-0.4847	-2.2889	-1.6584*	-1.6603	-0.5566	-2.0825
	(0.8823)	(0.9888)	(1.1667)	(1.4940)	(0.9026)	(1.2387)	(1.2041)	(1.4099)
N	5796	5796	5292	5292	7126	7108	3962	3860
Panel B: Chinese Aid								
ln($Chinese\,Aid_{\text{t-2}}$)	-0.1278	-0.1910	-0.3899	-0.1237	-0.1820	-0.3820	-0.2832	-0.1330
	(0.3711)	(0.5075)	(0.6393)	(0.6914)	(0.5441)	(0.6646)	(0.4656)	(0.6006)
N	3864	3864	3528	3528	4693	4681	2698	2626
Country-year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (7) and (8) of Table 3. In order to identify regions below/above the median of ethnic fractionalization and to identify whether any group in the region was part of the power-sharing agreement, we rely on Weidmann et al. (2010) and Vogt et al. (2015). Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.
Interpretation: Based on the literature on development aid and conflict, one may expect that ethnic fractionalization and power-sharing agreements may mediate the relationship. To address this question, we run a subsample analysis. We find that aid, if anything, is significantly negatively related to conflict. However, there is a fair bit of heterogeneity, suggesting room for future research to explore these results in more detail.

Table 28: FE - Heterogeneity – 2/2

	Autocracy		Democracy		1^{st} half of sampling period		2^{nd} half of sampling period	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: WB Aid								
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.7607	-1.6389	-0.2485	-0.3122	-1.0721	-1.0723	-0.3297	-1.0511
	(0.9096)	(1.0211)	(1.2122)	(1.5758)	(1.0010)	(1.1140)	(0.8528)	(0.9161)
N	9184	9184	3866	3866	6525	6525	6525	6525
Panel B: Chinese Aid								
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.1838	-0.0415	-0.6528	-0.7581	0.1394	0.0513	-0.2860	-0.4346
	(0.3419)	(0.3789)	(0.6380)	(0.8530)	(0.4313)	(0.4527)	(0.3891)	(0.4354)
N	5896	5896	2779	2779	4350	4350	4350	4350
Country-year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Results are based on subsample regressions, which correspond to the specifications in columns (7) and (8) of Table 3. Autocracy and democracy are distinguished based on Bjørnskov and Rode (2019). For the 1^{st} and 2^{nd} half, we split the sample before 2004 for the WB and before 2007 for China. The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Regressions account for (time-varying) exogenous controls, fixed effects for sub-national regions, and time trends. Time trends include linear and squared country-specific time trends as well as a linear regional trend. Standard errors in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Interpretation: Based on the literature on development aid and conflict, one may expect that political institutions may mediate the relationship and aid effects differ across time periods due to changes in development policy objectives. To address this question, we run a subsample analysis.

D Further Tests

D.1 Channels: Aid Sectors

Aid in different sectors could be more or less likely to fuel or calm down a conflict. We examine aid projects in eight subcategories with and without country-year FE.

Aid in different sectors exhibits different effects on conflict. Table 29 shows that there are positive coefficients of WB (Chinese) aid in a few categories, though statistically insignificant. The insignificant negative average effects in previous tables seem to be driven by significant conflict-reducing effects for the sectors "public administration," "health and other social services," "industry and trade" (WB only), and "transportation" (WB and China). A one standard deviation increase in WB aid for health and other social services is associated on average with a 1.3 to 1.4 percentage point reduction of the conflict likelihood – relative to the baseline likelihood of 12 %.

Regarding the transportation sector, a one standard deviation increase in Chinese (WB) transport sector aid is associated with a 0.4 to 0.5 (1.2, albeit insignificant with country-year FE) percentage points reduction of the conflict likelihood. This sector has many large-scale infrastructure projects with large disbursements in dollar terms. The negative effect suggests that high transportation costs were significant obstacles for exchange, consumption, public goods provision, and eventually economic growth (see also Berman and Couttenier, 2015; Storeygard, 2016). This seems to dominate both potentially negative effects on corruption (Isaksson and Kotsadam, 2018a) and disputes over land usage. It is in line with Bluhm et al. (2020a), who show that Chinese infrastructure projects reduce economic inequality and, hence, potential reasons for conflict.[25]

Overall, the heterogeneities across aid categories are a first explanation for the relatively broad confidence interval when studying the average effect of WB and Chinese aid. We find no significant conflict-fueling effect on any aid sector for neither donor. The overall negative relationship does not seem to mask strong conflict-fueling effects in certain sectors.

[25] Improvements in transportation infrastructure are likely linked to higher accessibility for the media and correlate with mobile phone coverage. This would induce an upward bias to our estimates (Weidmann, 2016; Von Borzyskowski and Wahman, 2019).

Table 29: FE – Aid Sectors and Conflict

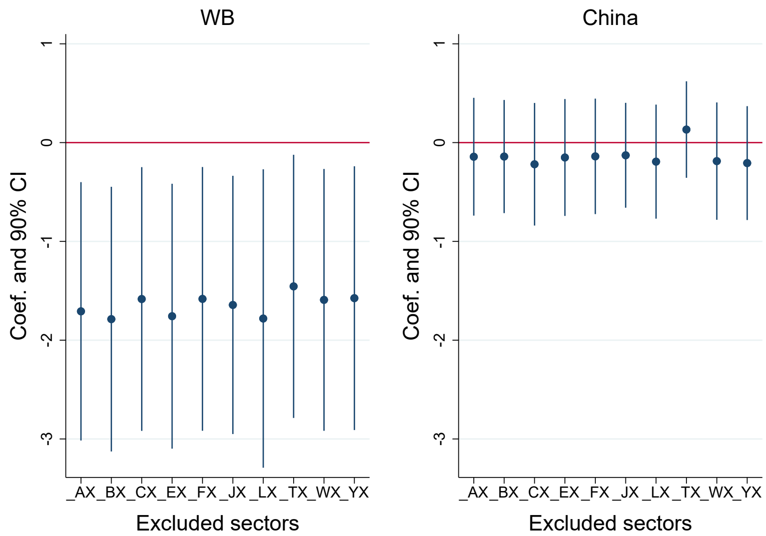
World Bank Aid Sectors - IV	AX	BX	CX	EX	FX	JX	LX	TX	WX	YX
Panel A: No Country-Year FE										
$\ln(World\,Bank\,Aid_{\text{t-1}})$	0.2239	-1.5685**	0.4415	0.1667	-0.4485	-1.3316**	0.1593	-1.2214**	-0.2669	-0.6798
	(0.5602)	(0.6966)	(0.5484)	(0.5875)	(0.4147)	(0.5815)	(0.5780)	(0.5685)	(0.6416)	(0.5633)
Panel B: Country-Year FE										
$\ln(World\,Bank\,Aid_{\text{t-1}})$	-0.4712	-2.2379***	0.0172	-0.1620	-0.4267	-1.4087*	-0.2141	-0.9391	0.0103	-1.2562*
	(0.6658)	(0.7984)	(0.6242)	(0.7661)	(0.6326)	(0.7571)	(0.6318)	(0.7415)	(0.7941)	(0.6443)
N	13050	13050	13050	13050	13050	13050	13050	13050	13050	13050
Chinese Aid Sectors - OLS	**AX**	**BX**	**CX**	**EX**	**FX**	**JX**	**LX**	**TX**	**WX**	**YX**
Panel C: No Country-Year FE										
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.2590	-0.3287	0.2701	-0.1152	.	-0.0307	0.3497	-0.4394*	-0.3603	0.5981
	(0.1650)	(0.2251)	(0.2026)	(0.2210)	(.)	(0.2930)	(0.2669)	(0.2586)	(0.2273)	(0.6554)
Panel D: Country-Year FE										
$\ln(Chinese\,Aid_{\text{t-2}})$	-0.1593	-0.2913	0.1956	-0.0672	.	0.0525	0.3223	-0.5340*	0.0457	0.5553
	(0.1914)	(0.2190)	(0.1925)	(0.2289)	(.)	(0.2805)	(0.2806)	(0.3104)	(0.2640)	(0.5794)
N	8700	8700	8700	8700	.	8700	8700	8700	8700	8700

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5). The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Regressions account for (time-varying) exogenous controls and time trends. Time Trends include linear and squared country-specific time trends as well as a linear regional trend. AX - "Agriculture, fishing, and forestry" BX - "Public Administration, Law, and Justice" CX - "Information and communications" EX - "Education" FX - "Finance" JX - "Health and other social services" LX - "Energy and mining" TX - "Transportation" WX - "Water, sanitation and flood protection" YX - "Industry and Trade" The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. Standard errors are in parentheses, two-way clustered at the country-year and regional level: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Interpretation: One may be concerned about the existence of a certain conflict-increasing type of aid that is masked in our overall aid measure. To address this concern, we find that aid in any sector is, if anything, significantly negatively related to conflict. However, there is a fair bit of heterogeneity. This also highlights that some types of aid may have a different effect in the short and long run. Generally, this suggests ample room for future research to explore these results in more detail.

One may be concerned that donors take into account that certain types of aid are more likely to induce conflict and thus endogenously allocate aid across sectors. For example, Child (2018) finds that aid in the education sector is more likely to fuel conflict due to ideological backlash.[26] We consider this concern in Figure 22, where we run our preferred specification from column 8 of Table 3 and drop one by one aid from one specific sector. Results remain qualitatively unchanged and indicate a *conflict-reducing* effect for the World Bank and a *neutral* effect for Chinese aid, irrespective of the sectors excluded. The only notable finding arises when excluding Chinese transport aid. Although turning positive, the average coefficient remains insignificant and does not change the overall finding of a negative to neutral aid effect. The finding is in line with the significant negative effect of Chinese transport aid in Table 29, pointing to a conflict-reducing relationship with transport aid.

Figure 22: Aid Sectors – Jack-Knife Test



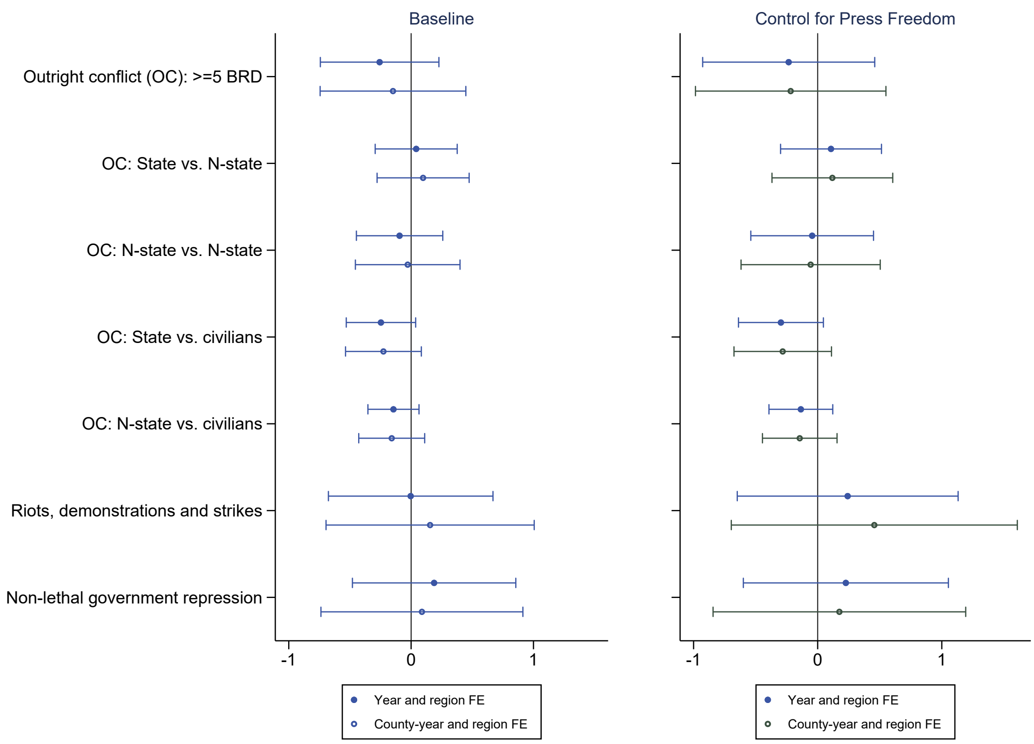
Notes: Regressions exclude aid in specific sectors, where labels refer to the following: AX - “Agriculture, fishing, and forestry” BX - “Public Administration, Law, and Justice” CX - “Information and communications” EX - “Education” FX - “Finance” JX - “Health and other social services” LX - “Energy and mining” TX - “Transportation” WX - “Water, sanitation, and flood protection” YX - “Industry and Trade.” The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$.
The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors are in parentheses, two-way clustered at the country-year and regional level. Conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 due to the lag structure. Regressions account for (time-varying) exogenous controls, country-year fixed effects, linear and squared country-specific time trends as well as a linear regional trend. Confidence intervals are based on standard errors, which are two-way clustered at the country-year and regional level.

[26]This is supported by data from the World Bank's Independent Evaluation Group which shows that education projects in (post-)conflict settings are less successful (see Chauvet, Collier and Duponchel (2010)).

D.2 Potential Media Bias for Chinese Aid Data - Controlled for Press Freedom

Table 30 adds the interaction of press freedom and region-level fixed effects as a further endogenous control variable for the other conflict outcomes, which we consider in our paper. Coefficients remain insignificant and are comparable to the main results in Table 3 suggesting that press freedom would not systematically bias reported aid flows and the relationship between aid and conflict.

Figure 23: Results for China Controlled for Press Freedom Interacted With Region FE



Notes: The figure shows coefficient plots of individual FE regressions of our binary conflict incidence indicator (100 if BRD≥5, 0 if BRD<5) on aid. Aid is measured in standard deviations of $ln(aid + 0.01\,\text{USD})$. Hence, the coefficients reflect the effect of a one standard deviation change in Chinese aid. The sample includes first-order sub-national regions in African countries for the 2000-2012 period (China). Conflicts are considered for Chinese aid from 2002 to 2014 due to the lag structure. Exogenous (time-varying) controls are included in all regressions. The Time Trends included consist of linear and squared country-specific time trends as well as linear regional time trends. "State vs. N-State" refers to state-based violence against non-government actors, "N-State vs. N-State" refers to non-government violence against other organized non-state groups. "State vs. Civilians" and "N-State vs. Civilians" refer to one-sided violence versus civilians by the government and non-government actors, respectively. The categories are mutually exclusive. "Riots, Demonstrations, and Strikes" and "Non-lethal Government Repression" are binary protests and government repression incidence indicators, taking on the value of 100 if there was at least one event in the respective category. 90% confidence intervals are based on standard errors, which are two-way clustered at the country-year and regional level. Full results are displayed in tables 30 and 31.

Table 30: Full Regression Results - Controlled for Country Level Press Freedom Interacted With Region FE (1)

	Intensity 1		State vs. N-State		N-State vs. N-State		State vs. Civilians		N-State vs. Civilians	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln($Chinese\ Aid_{\text{t-2}}$)	-0.0582	-0.0542	0.0267	0.0295	-0.0111	-0.0141	-0.0738	-0.0702	-0.0337	-0.0360
	(0.1049)	(0.1162)	(0.0616)	(0.0737)	(0.0748)	(0.0849)	(0.0518)	(0.0595)	(0.0389)	(0.0455)
N	8261	8261	8261	8261	8261	8261	8261	8261	8261	8261
Country-Year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The dependent variable is a binary conflict incidence indicator (100 if BRD$\geq$5, 0 if BRD$<$5) in columns 1 and 2, which is considered for different actors across columns 3 to 10. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. The sample includes first-order sub-national regions in African countries for the 2000-2012 period (China). Specifications are identical to Table 3, columns 7 and 8. Standard errors are in parentheses, two-way clustered at the country-year and regional level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 31: Full Regression Results - Controlled for Country Level Press Freedom Interacted With Region FE (2)

	Riots, Demonstrations, and Strikes		Non-lethal Repression	
	(1)	(2)	(3)	(4)
ln($Chinese\ Aid_{\text{t-2}}$)	0.0602	0.1137	0.0566	0.0436
	(0.1347)	(0.1745)	(0.1250)	(0.1542)
N	8261	8261	8261	8261
Country-Year FE	No	Yes	No	Yes

Notes: Columns 1 and 2 consider a binary indicator which equals 100 for non-lethal repressions and columns 3 and 4 a binary indicator which equals 100 for riots, demonstrations and strikes. The treatment refers to standard deviations of $ln(aid + 0.01\,\text{USD})$. The sample includes first-order sub-national regions in African countries for the 2000-2012 period (China). Specifications are identical to Table 3, columns 7 and 8. Standard errors are in parentheses, two-way clustered at the country-year and regional level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

D.3 Selection-on-unobservables

We also consider coefficient stability along the lines of Altonji et al. (2005) and Oster (2019) for all stability outcomes used throughout the paper. Table 32 provides the ratio on how high selection-on-unobservables would need to be to make the coefficient change its sign, when comparing β_l from a regression without considering any covariates as in column 1 of Table 3 and β_f with the full set of exogenous controls and fixed effects from column 8 of Table 3. We consider not the additional controls from column 9 since those may (re-)introduce potential endogeneity bias.

Table 32: Using Selection-on-unobservables to determine Selection Bias

Covariate set from Column (1) vs Column (8)				
Panel A: World Bank Aid	β_l (Standard Error)	β_f (Standard Error)	Identified	Null β-set reject?
Intensity 1	0.01 (0.72)	-1.65 (0.69)	[-3.29; 0.01]	No
State vs. N-State	-0.09 (0.50)	-1.27 (0.54)	[-2.41;-0.09]	Yes
N-State vs. N-State	0.51 (0.46)	-0.73 (0.44)	[-2.32; 0.51]	No
State vs. Civilians	-0.38 (0.30)	-0.35 (0.34)	[-0.38;-0.30]	Yes
N-State vs. Civilians	0.29 (0.37)	-0.91 (0.41)	[-2.25; 0.29]	No
Repression	1.16 (0.64)	-0.72 (0.62)	[-3.22; 1.16]	No
Riots, Demonstrations, & Strikes	1.20 (0.96)	-0.09 (0.77)	[-1.49; 1.20]	No
Panel B: Chinese Aid	β_l (Standard Error)	β_f (Standard Error)	Identified β-set	Null reject?
Intensity 1	-0.09 (0.28)	-0.14 (0.33)	[-0.18;-0.09]	Yes
State vs. N-State	-0.01 (0.18)	0.05 (0.25)	[-0.01; 0.10]	No
N-State vs. N-State	-0.16 (0.19)	0.01 (0.22)	[-0.16; 0.17]	No
State vs. Civilians	-0.06 (0.13)	-0.25 (0.17)	[-0.52;-0.06]	Yes
N-State vs. Civilians	-0.09 (0.13)	-0.13 (0.18)	[-0.17;-0.09]	Yes
Repression	3.01 (0.61)	0.00 (0.33)	[-3.36; 3.01]	No
Riots, Demonstrations, & Strikes	4.14 (0.76)	0.06 (0.40)	[-3.84; 4.14]	No

Notes: Selection-on-unobservables estimation based on Oster (2019). Estimates refer to the set of covariates from Table 3. Click here to go back to section 5.1.